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BASIC SCIENCE BUDGET AND SSC

HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON
ENERGY AND NATURAL RESOURCES
UNITED STATES SENATE

ONE HUNDREDTH CONGRESS

SECOND SESSION

ON THE

DEPARTMENT OF ENERGY'S FISCAL YEAR 1989 REQUEST FOR THE SUPERCONDUCTING SUPER COLLIDER AND THE BASIC SCIENCE BUDGET

APRIL 12, 1988

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BASIC SCIENCE BUDGET AND SSC

APRIL 12, 1988

U.S. SENATE,
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT,
COMMITTEE ON ENERGY AND NATURAL RESOURCES,
Washington, DC.

The subcommittee met, pursuant to notice, at 9:30 a.m., in room SD-366, Dirksen Senate Office Building, Hon. Wendell H. Ford presiding.

OPENING STATEMENT OF HON. WENDELL H. FORD, U.S. SENATOR FROM KENTUCKY

Senator FORD. Good morning, ladies and gentlemen. On April 7, 1987, almost exactly one year ago, the Subcommittee on Energy and Research and Development held its first hearing on the Superconducting Super Collider.

At that time I said that the money for the SSC would have to come from an add-on to, or a corresponding cut in, the Department of Energy's programs in research and development established by this Committee.

In this Committee, we have been trying to support these R&D programs against cuts proposed by the Administration. Now the Administration proposes to increase funding for the SSC from $25 million in fiscal year 1988 to $363 million in fiscal year 1989, but to cut R&D for energy conservation by 48 percent, fossil energy by 49 percent—and I am not going to allow that—and solar energy, renewable energy by 16 percent.

It is less obvious, but the proposal to increase SSC funding has also operated to delay other scientific projects. I said last year that something would have to give.

The Administration has its own answer to the dilemma. The Administration wants everything else to give so we can have the SSC. I would volunteer the observation that this is not very realistic.

If the SSC is set in competition with all of these other programs, the SSC is not likely to get the money that is needed to make it a viable project. If this Administration wants to have an SSC, it is going to need to come up with a better plan to fund the project than anything we have seen since last year's hearings.

Maybe the game is just to propose the project with all the money, not get it, and then blame Congress for failing to fund it. If that is the case, the scientists here to testify are just pawns in that game. If it is not the case, we need a better explanation of where the money for the SSC will come from.

I hope we can get some answers today. At least we can know what we are up against as it relates to the SSC. We hope that the witnesses today will be brief. I think that we have some pros and some cons as it relates to the funding of this operation.

Dr. Decker, your's is 67 pages, I think, I hope you will not go through all of that for the Committee. I perused it last night and the staff has read it in detail.

And before I go any further, this gentleman to my right only looks old. Today is his birthday, and he is celebrating the 7th anniversary of his 39th birthday, I understand. I wanted that to be included in the record, young lady, because it is probably the most important thing that will be given here this morning. [Laughter.]

I have a couple statements for the record, one from the distinguished Chairman of this Committee, Senator Johnston, and a statement that Senator Gramm of Texas has asked that we include in the record at this point.

[The prepared statements of Senators Ford, Johnston, and Gramm follow:]

STATEMENT OF SENATOR WENDELL H. FORD
CHAIRMAN, SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

HEARING ON THE SUPERCONDUCTING SUPER COLLIDER
AND BASIC SCIENCE PROGRAMS
DEPARTMENT OF ENERGY FY89 BUDGET REQUEST

APRIL 12, 1988

ON APRIL 7, 1987, ALMOST EXACTLY ONE YEAR AGO, THE SUBCOMMITTEE ON ENERGY AND RESEARCH AND DEVELOPMENT HELD ITS FIRST HEARING ON THE SUPERCONDUCTING SUPER COLLIDER. AT THAT TIME, I SAID THAT THE MONEY FOR THE SSC WOULD HAVE TO COME FROM AN ADD-ON TO, OR A CORRESPONDING CUT IN, THE DEPARTMENT OF ENERGY PROGRAMS IN RESEARCH AND DEVELOPMENT ESTABLISHED BY THIS COMMITTEE. IN THIS COMMITTEE WE HAVE BEEN TRYING TO SUPPORT THESE R&D PROGRAMS AGAINST CUTS PROPOSED BY THE ADMINISTRATION.

NOW THE ADMINISTRATION PROPOSES TO INCREASE FUNDING FOR THE SSC FROM $25 MILLION IN FY 1988 TO $363 MILLION IN FY 1989, BUT TO CUT R&D FOR ENERGY CONSERVATION BY 48%, FOSSIL ENERGY BY 49%, AND SOLAR ENERGY/RENEWABLE ENERGY BY 16%.

IT IS LESS OBVIOUS, BUT THE PROPOSAL TO INCREASE SSC FUNDING HAS ALSO OPERATED TO DELAY OTHER SCIENTIFIC PROJECTS. EXAMPLES INCLUDE THE CONTINUOUS ELECTRON BEAM ACCELERATOR FACILITY (CEBAF) IN VIRGINIA WHOSE FUNDING IS REDUCED AND THE RELATIVISTIC HEAVY ION COLLIDER IN NEW YORK WHOSE CONSTRUCTION WILL NOT BE STARTED.

I SAID LAST YEAR THAT SOMETHING WOULD HAVE TO GIVE. THE ADMINISTRATION HAS ITS OWN ANSWER TO THIS DILEMMA. THE

ADMINISTRATION WANTS EVERYTHING ELSE TO GIVE SO WE CAN HAVE THE SSC.

I WOULD VOLUNTEER THE OBSERVATION THAT THIS IS NOT VERY REALISTIC. IF THE SSC IS SET IN COMPETITION WITH ALL THESE OTHER PROGRAMS, THE SSC IS NOT LIKELY TO GET THE MONEY THAT IS NEEDED TO MAKE IT A VIABLE PROJECT.

IF THIS ADMINISTRATION WANTS TO HAVE AN SSC, IT IS GOING TO NEED TO COME UP WITH A BETTER PLAN TO FUND THE PROJECT THAN ANYTHING WE'VE SEEN SINCE LAST YEAR'S HEARING.

MAYBE THE GAME IS JUST TO PROPOSE THE PROJECT AND BLAME CONGRESS FOR FAILING TO FUND IT. IF THAT'S THE CASE, THE SCIENTISTS HERE TO TESTIFY ARE JUST PAWNS IN THE GAME. IF IT IS NOT THE CASE, WE NEED A BETTER EXPLANATION OF WHERE THE MONEY FOR THE SSC WILL COME FROM.

I HOPE WE GET SOME ANSWERS TODAY SO, AT THE LEAST, WE CAN KNOW WHAT WE ARE UP AGAINST IN THIS MATTER.

STATEMENT OF SENATOR J. BENNETT JOHNSTON

HEARING ON THE SUPERCONDUCTING SUPER COLLIDER
AND THE BASIC SCIENCE PROGRAMS
DEPARTMENT OF ENERGY FY89 BUDGET REQUEST

APRIL 12, 1988

I CONGRATULATE SENATOR FORD FOR HOLDING THIS HEARING TODAY ON THE SUPERCONDUCTING SUPER COLLIDER AND THE IMPACT THIS PROJECT WILL HAVE ON THE BUDGET OF THE DEPARTMENT OF ENERGY FOR BASIC SCIENCE.

THIS ISSUE HAS RIPENED IN THE PAST YEAR, AND THE ESSENTIAL CHOICES ARE BECOMING CLEARER.

THE NUMBER OF POTENTIAL SITES HAS BEEN NARROWED TO SEVEN. I UNDERSTAND THAT A SINGLE PREFERRED SITE WILL BE CHOSEN IN NOVEMBER, AFTER THE COMING ELECTIONS. THE PURELY PAROCHIAL SUPPORT FOR THE SSC THAT HAD BEEN BASED ON A HOPE, HOWEVER SLIGHT, THAT THE PROJECT WOULD "BE LOCATED IN MY STATE" IS NOW GREATLY REDUCED. A DECISION TO SPEND $5 OR MORE BILLION DOLLARS ON THE PROJECT WILL BE MADE, IF IT IS MADE, ON GROUNDS OF NATIONAL RATHER THAN PAROCHIAL INTEREST.

THE SCIENTIFIC COMMUNITY SEEMS TO BE WAKING UP TO THE FACT THAT FUNDING THE PROJECT WILL INVOLVE PAINFUL TRADEOFFS WITHIN THE DOE SCIENCE BUDGET. THERE ARE FEWER CALLS TO FUND THE SSC BY

REDUCING THE BUDGET FOR THE STRATEGIC DEFENSE INIATIVE OR BY CAPPING COST OF LIVING INCREASES OR BY REPEALING THE AUTHORIZATION FOR THE APPALACHIAN REGIONAL COMMISSION. THE SSC IS A SCIENCE PROJECT UNDER THE DOE. ITS FUNDS MUST COME FROM THE SCIENCE BUDGET OF DOE.

AS A RESULT, SOME SCIENTISTS ARE NOW COMING FORWARD TO SUGGEST PRIORITIES IN SCIENCE FUNDING. THERE IS A REALIZATION THAT ALL THESE FACILITIES WILL NOT BE FUNDED OUT OF "NEW MONEY'. THERE IS NO "NEW MONEY". THERE IS ONLY ONE KIND OF MONEY. WHEN THE FEDERAL GOVERNMENT SPENDS IT, THE DEFICIT GOES UP, AND WE DON'T WANT THAT.

THE AMOUNT WE CAN SPEND IS LIMITED, THEREFORE WE NEED PRIORITIES.

FINALLY, WE ARE BEGINNING TO SEE THAT THIS IS REALLY A MATTER OF HOW WE ARE GOING TO FUND ANY LARGE NEW CAPITAL IMPROVEMENT IN OUR SCIENTIFIC ENTERPRISE. WE HAVE AN UNDENIABLE NEED FOR A STRONG NATIONAL SCIENTIFIC ENTERPRISE. BUT WE ARE NOW IN A POSITION OF WANTING MORE THAN WE CAN PAY FOR.

WE HAD BETTER FIND SOME NEW WAYS TO PAY FOR WHAT WE WANT. WE ALSO HAD BETTER DO A BETTER JOB OF FIGURING WHAT TO DO WHEN, AFTER ALL WAYS OF PAYING HAVE BEEN TRIED, WE STILL DON'T HAVE ENOUGH MONEY FOR EVERY PROJECT THAT IS WORTHY.

Phil Gramm

STATEMENT OF U.S. SENATOR PHIL GRAMM

ON DOE'S FY 1989 BUDGET REQUEST FOR THE SUPERCONDUCTING SUPER COLLIDER

BEFORE THE ENERGY RESEARCH AND DEVELOPMENT SUBCOMMITTEE

OF THE SENATE COMMITTEE ON ENERGY AND NATURAL RESOURCES

April 12, 1988

Mr. Chairman and Members of the Subcommittee, I appreciate the opportunity to submit this statement today in support of the Superconducting Super Collider (SSC). I would like to offer my support for full funding in FY 1989 for construction of the SSC.

The SSC, which will keep this country on the cutting edge of high energy physics research, is extremely important to this nation and our national goal of U.S. leadership in science and technology. The Administration has requested $363 million in FY 1989 for initiating construction of the SSC. Everything that is possible should be done to accomodate this funding request.

Mr. Chairman, I believe strongly that federal dollars should be directed into priority programs such as the SSC, programs that are an investment in our future. This science project will allow this country to emerge as the recognized leader in new technology as we enter the next century. The knowledge to be gained from the SSC is invaluable and will make the United States the world's centerpiece for particle physics research and advanced accelerator technology. The scientists are eager to have this facility built and to begin their work. We should move ahead

and fully fund construction of the SSC so as not to delay our opportunity to be the world's leader in high energy physics.

I encourage you and Members of your Committee to support the President's budget request for the SSC.

Senator FORD. So, Dr. Decker, let us move on with brevity and dispatch.

STATEMENT OF DR. JAMES F. DECKER, ACTING DIRECTOR, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY

Dr. DECKER. Thank you, Mr. Chairman. I am here today to discuss the fiscal year 1989 budget request for the Office of Energy Research.

The Office of Energy Research supports long-range fundamental and applied research that provides new knowledge and develops the scientific base necessary for energy and other technologies.

Like other long-range research programs, these are investments which are certain to increase our scientific knowledge, and will have economic benefits in the future. These programs include research in all fields of the natural sciences and represent the largest Federal support in the physical sciences.

For fiscal year 1989, the energy research budget request is approximately $2.4 billion and is a major component of the Department's research and development commitment.

Our fiscal year 1989 request includes funding to preserve the core capabilities of our research programs, to operate our scientific facilities, and for a few selected enhancements to take advantage of new research opportunities.

First I will discuss the High Energy Physics Program and the Superconducting Super Collider. I understand that you are, as you have indicated, particularly interested in the Super Collider, and I will describe our request for this project in more detail than other parts of the program.

Senator FORD. That is fine. Thank you, sir.

Dr. DECKER. The field of high energy physics seeks to find the fundamental building blocks of matter and the forces that govern their behavior. This is the most fundamental area of basic research and is a primary responsibility of the Department.

Our fiscal year 1989 request emphasizes exploiting the enhanced capabilities of our recently completed collider facilities at Fermilab and Stanford. We will also continue construction of large detectors, the D-Zero at Tevatron at Fermilab, and the SLD at Stanford, to keep these accelerators in the forefront during the early 1990s.

Our request also includes funds to complete Fermilab's central computing upgrade and to continue construction of the Alternating Gradient Synchrotron Booster at Brookhaven. That will be completed in 1991.

Of course, the most important for the future of high energy physics is the Superconducting Super Collider. High energy physicists have made great progress in understanding the fundamental constituents of matter by building a series of more powerful particle accelerators.

The new collider facilities at Fermi Laboratory and Stanford will allow us to continue to make progress in this field until the mid-1990s, when a new facility will be needed.

At that time there will remain a number of unanswered questions that require a particle accelerator as powerful as the Superconducting Super Collider to explore.

The Superconducting Super Collider will allow physicists to study proton-proton collisions at energies up to 40 trillion electron volts. That is about 20 times more powerful than the energies available at the Tevatron at Fermi Laboratory today. The SSC is a racetrack-shaped accelerator that is some 53 miles in circumference.

During the past year, the Department has continued with research and development primarily on superconducting magnets.

The Department has also moved forward with site selection, and with the help of the National Academy of Sciences and the National Academy of Engineering, has developed the best qualified list consisting of seven sites. The Department plans to announce the preferred site at the end of November.

For fiscal year 1989, the Department is requesting $363 million for the Superconducting Super Collider. This request includes $80 million for research and development consisting of $64 million in operating funds and $16 million in equipment funds. Most of this research and development funding will be used for superconducting magnet development and testing.

We are also requesting $283 million to begin construction. Construction funds will be devoted to detailed design of technical systems and conventional facilities, long lead critical path procurements, and a modest amount of site preparations.

In order to share the costs of this world-class facility, the Department is aggressively seeking international participation in the SSC, either with cost-sharing or with contributions in kind.

And I believe that substantial in-kind contributions are likely from several countries, but commitments are not expected until Congress approves the project and approves substantial support for it.

I know that there has been much concern about the impact of the SSC on other science programs. However, let me assure you that the SSC did not have an adverse effect on any scientific programs in the Department.

The SSC funding was considered as a separate item and was added to the Department's 1989 budget target by the Office of Management and Budget.

The Superconducting Super Collider is an expensive project. However, I want to point out that the benefits from the SSC will not be limited to its site, but will be for the entire country.

Hundreds of businesses and thousands of American workers will participate in its construction and operation. Scientists from over 100 universities across the Nation will participate in its research.

It will provide inspiration to the next generation of scientists throughout the country. It will show the country's determination and commitment to stay at the leading edge of science.

Now I want to briefly mention highlights from some of the other programs of the Office of Energy Research. First, nuclear physics.

We are requesting funds to continue construction of the Continuous Electron Beam Accelerator Facility, with completion of this project expected in 1993.

We are also requesting funding to continue nuclear physics research in existing facilities and $6 million to continue research and

development on a possible new device, the Relativistic Heavy Ion Collider at Brookhaven National Laboratory.

Next I want to turn to the Magnetic Fusion Energy Program. This Program is focused primarily on the physics of fusion plasmas. The Program's major experiment is the Tokamak Fusion Test Reactor, known as TFTR, at Princeton University.

We are completing preparations for the deuterium-tritium breakeven experiment in TFTR. Equipment installation will be finished during fiscal year 1989 and early 1990, and actual deuterium-tritium experiments will begin in January, 1991.

This year, for the first time, the plasma conditions in TFTR exceeded the original design goals, giving us confidence that the deuterium-tritium experiments will be productive.

To move beyond this energy breakeven experiment in TFTR, our 1989 request includes construction funds for a Compact Ignition Tokamak that would attain ignition conditions in which the fusion reaction is self-sustained.

Now, this is a critical remaining science issue which we must understand before we can consider engineering development of fusion energy.

Next I would like to turn to the Basic Energy Sciences Program. This is a broad research program that underlies many of the Department's more applied programs.

In addition to funding research, this Program operates a number of user facilities such as synchrotron light sources, high-resolution electron microscopes, research reactors and so forth. These facilities are used by scientists from universities and industry, as well as our national laboratories.

Research on high-temperature superconductors provides a good example of how researchers and user facilities in this Program can be mobilized to exploit new discoveries.

Atomic structure of these new materials has been determined using neutrons from our research reactors and photons from our synchrotron light sources. We are confident that this research will lead to an understanding of why these materials are superconductors and how to improve their properties for application.

In the area of new facilities, our request includes funding to begin construction of the 7 Gev Synchrotron Light Source at the Argonne National Laboratory.

The high-intensity, higher energy X-rays to be uniquely available from this source will open up new research opportunities in materials, biology, chemistry, and medicine.

The last program that I would like to mention is biological and environmental research. Our request will expand the genome program that has, as its ultimate goal, the complete characterization of the human genome on the molecular level.

The Department's unique role will be to develop technologies and other resources to map and ultimately sequence the genome at much lower cost than present-day technologies would imply.

Our request also includes funding to expand research into the health effects of radon and to expand research in subsurface microbiology.

Mr. Chairman, that concludes my oral summary. I will be glad to answer any questions you may have.

[The prepared statement of Dr. Decker follows:]

Statement of James F. Decker

Acting Director

Office of Energy Research

Department of Energy

before the

Subcommittee on Energy Research and Development

of the

Senate Committee on Energy and Natural Resources

April 12, 1988

Mr. Chairman and Members of the Subcommittee:

It is a pleasure to appear here today to present the FY 1989 budget request for the programs supported by the Office of Energy Research (ER). These programs are: High Energy Physics, Nuclear Physics, Basic Energy Sciences, Magnetic Fusion Energy, Biological and Environmental Research, Energy Research Analysis, University Research Support, University Research Instrumentation, and Multiprogram Energy Laboratories-Facilities Support and supporting program direction. Funding for these programs in FY 1989 is being requested under the three separate appropriations shown in Table 1.

The new Basic Research User Facilities (BRUF) appropriation includes funding for construction, operation, and maintenance of the Office of Energy Research's major "user facilities." Construction and operation of these large, complex and expensive facilities requires sustained, central federal support since they are far too large and costly to expect non-federal research institutions to provide such facilities for the benefit of the scientific community. These facilities provide a unique national resource for forefront research in areas related to the use and production of energy. Their continued operation is essential to ensure a continuous support of new knowledge, trained personnel and new directions for the energy well-being of the U.S. and to support the technology base that is vital to the Nation's economic future. It is important to note that the BRUF funds, which include operating expenses, capital equipment and construction, provide only for the construction, operation and maintenance of the facilities. The research conducted in these facilities continues to be funded under the Energy Supply R&D or General Science and Research appropriations.

Table 1
Energy Research Programs by Appropriation
FY 1989

Energy Supply R&D	General Science and Research	Basic Research User Facilities (BRUF)
Basic Energy Sciences	High Energy Physics	BRUF-High Energy Physics
Magnetic Fusion Energy	Nuclear Physics	BRUF-Nuclear Physics
Biological & Environmental Research	General Science Program Direction	BRUF-Superconducting Super Collider
University Research Support		BRUF-Basic Energy Sciences
University Research Instrumentation		
Energy Research Analysis		
Advisory & Oversight Program Direction		
Policy and Management		
Multiprogram Energy Laboratories-Facilities Support		

The FY 1989 budget request for ER's programs is shown in Table 2. The total amount requested in FY 1989 is $2.4 billion, an increase of $0.3 billion over the FY 1988 appropriation of $2.1 billion. This request reflects the Reagan Administration's strong commitment to support progress in all areas of science with the goal of enhancing the economic and technological health of the United States as well as the quality of our lives.

HIGH ENERGY PHYSICS

The main goal of high energy physics research is to develop new knowledge and a better understanding of the nature of matter and energy as well as the basic forces that govern all processes in nature. One of the major current objectives of high energy physics research is to develop a unified description, or single theory, that would describe all the interactions of matter and energy in the universe. Another major objective is to search for the most fundamental constituents of matter. As with any exploratory field of research, one can also anticipate new and totally unexpected discoveries.

In recent years, high energy physicists have developed an increasingly better understanding of the forces of nature and the unifying connections among them. The four known fundamental forces are the electromagnetic force, the weak force, the strong force, and the gravitational force. The search for a unified, or single, theory has been a reliable guide to physics research. It is a search for linkages between apparently diverse phenomena in an effort to discover underlying simplicity. A part of the progress in high energy physics has been the conceptual unification of the weak nuclear force and

Table 2
Office of Energy Research
Summary by Program
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Energy Supply R&D			
Basic Energy Sciences	$432.3	$437.2	$375.6
Magnetic Fusion Energy	341.6	335.0	360.0
Biological & Environmental Research	191.1	269.3	249.0
University Research Support	15.6	18.4	15.8
University Research Instrumentation	4.9	5.0	5.0
Energy Research Analysis	2.0	3.7	3.8
Advisory & Oversight Program Direction	2.5	3.2	3.3
Policy and Management	0.6	0.7	0.7
Multiprogram Energy Laboratories-Facilities Support	56.7	53.6	65.9
Subtotal	$1,047.3	$1,126.1	$1,079.1
General Science & Research			
High Energy Physics	$201.9	$217.4	$220.4
Nuclear Physics	122.2	134.9	141.4
General Science Program Direction	2.5	2.8	3.2
Subtotal	$326.6	$355.1	$365.0
Basic Research User Facilities			
Basic Energy Sciences	$ 87.2	$125.5	$140.5
Nuclear Physics	92.7	110.2	122.7
High Energy Physics	273.4	314.2	346.4
Superconducting Super Collider	20.0	25.0	363.0
Subtotal	$473.3	$574.9	$972.6
Total ER	$1,847.2	$2,056.1	$2,416.7

electromagnetic forces into a single "electroweak" framework. In addition, a theory of the strong force has emerged which is consistent with experimental results. There is increasing anticipation that the unification of the strong nuclear and the electroweak forces into one "grand unified" framework may be possible, thus reducing the apparent number of forces in nature to two. The ultimate goal is a "super-grand" unification that would bring all of the forces in nature into a single unified framework.

This picture of nature in terms of the electroweak and strong forces and a small set of fundamental constituents is known as the Standard Model. The most significant recent experimental results all reinforce the predictions of the Standard Model. Although no deviations from the Standard Model have been found, scientists believe that it cannot be the final answer. For example, it does not explain the origin of mass and it does not include gravity. Experiments using newly available research capabilities at Fermi National Accelerator Laboratory (Fermilab) and the Stanford Linear Accelerator Center (SLAC) are testing the Standard Model and searching for phenomena that do not conform to that Model, as well as for phenomena predicted by newer theories.

The ability to carry out forefront exploratory research on the physics frontier is critically dependent on the experimental capabilities of the accelerators, colliding beam devices and large particle detectors. Provision of upgraded and new facilities on a timely basis, thus, is an important consideration. There are three DOE supported accelerator centers, Fermilab, Brookhaven National Laboratory (BNL), and SLAC, each of which provides world-unique capabilities. Two major upgrades of U.S. High Energy Physics

facilities, the Stanford Linear Collider (SLC) and the Fermilab Tevatron Collider, which recently came into operation, will be available to produce strong research output in FY 1989. These facilities will keep the Program highly competitive and at the cutting edge for the next several years. To maintain an internationally competitive program at the scientific frontier in the mid 1990's, activities leading to the construction of a new major facility, the Superconducting Super Collider (SSC), have been initiated.

University scientists carry out over three fourths of the experimental and theoretical research in the field, much of it at the major accelerator facilities. Universities not only provide intellectual leadership for the field, but also play a major role in the training of highly skilled scientists and engineers. In fact, many of these experts are eagerly sought by industry and other scientific disciplines. As they apply their high energy physics training in other areas, these scientists contribute significantly to the transfer of technology to other fields. For example, the 1979 Nobel Prize in Medicine was shared by Alan Cormack who applied techniques from his experience in bubble chamber data analysis in the High Energy Physics program to develop the computerized axial tomography technique for medical diagnosis. This technology, commonly known as the CAT scan, is in widespread use today.

Before turning to a discussion of the FY 1989 budget request, I would like to summarize some of the progress made in 1987 at the major High Energy Physics research facilities. At the Tevatron at Fermilab, the new capabilities for proton-antiproton colliding beam experiments achieved record high values of beam energy and intensity, with both beams operating at 900 billion electron

volts (GeV) for colliding beam experiments. The antiproton source set a new world record by repeatedly accumulating more than 10 billion antiprotons per hour. Progress also continued on the two major colliding beam detector facilities that will measure the results of collisions at the Tevatron's new energy levels. During its initial physics run at Fermilab, the Collider Detector (CDF) fully met all its technical goals. Besides completing its first checkout and engineering run, CDF physicists were able to record more than 400,000 triggered events on about 500 magnetic tapes--enough data to ensure new insight into the unexplored terrain of antiproton-proton interactions at the 1.8 trillion electron volts (TeV) reaction energy. In addition, fabrication of the massive, 5,500 ton D-Zero detector for the Tevatron collider advanced well in 1987 and remains on schedule for full operations in 1990 when it will take data simultaneously with the CDF and, thereby, double the Tevatron's collider physics productivity.

The just-completed Tevatron fixed target program run has been the most productive one since the superconducting accelerator was first brought into operation in 1983. In just over 30 weeks of operation at its world record beam energy of 800 GeV for fixed target experiments, the Tevatron has accelerated almost two billion billion (2×10^{18}) protons. Eleven major fixed target experiments, involving about a thousand physicists and graduate students, have been completed. Almost a hundred Ph.D. theses at several dozen major universities are anticipated based on analyses of the data collected during these experiments. Four new fixed target beam lines were commissioned and brought into operation for data collection during this period. In addition, essential calibration tests of apparatus and R&D efforts were

successfully carried out in two test beams which operated parasitically during the fixed target physics run. Extensive studies and commissioning work were also carried out using the Fermilab Antiproton Accumulator ring in a new mode of operation in which it can provide a beam of antiprotons for a fixed target experiment when not needed for the collider physics program. In summary, we have just concluded the most productive and successful fixed target operating period ever conducted at Fermilab.

In 1987, the Stanford Linear Accelerator Center's ability to study collisions of electrons with positively charged particles (positrons) significantly increased with the completion of the Stanford Linear Collider (SLC) project. Beam testing and tune-up are in progress now. Initial operation for research, at a reaction energy of 94 GeV, is expected to begin very soon. The higher energies available in the SLC will enable scientists to conduct important new experiments, particularly with regard to the electroweak force. In preparation for first collisions of 47 GeV electrons and positrons, the upgraded Mark II Detector was installed in the SLC interaction region and tested with cosmic rays during 1987. It is now ready to capture results of collisions in the SLC's unprecedented electron-positron energy range.

Substantial progress was made in the fabrication of the Stanford Large Detector (SLD) during 1987. The SLD is a full-scale detector, designed specifically to optimize the physics output from the electron-positron collisions in the SLC.

At Brookhaven National Laboratory the major technical design and the conventional construction design efforts for the Alternating Gradient Synchrotron (AGS) Accumulator/Booster were completed and the tooling and initial procurements for the 202-meter ring of beam-bending magnets began. This additional particle injector to the AGS will increase the intensity of both the AGS proton beams by 400 percent and the intensity of polarized proton beams by 2000 percent. This enhancement will permit researchers at Brookhaven to explore previously inaccessible areas of the subatomic world and will significantly increase the rate of data collection.

FY 1989 HIGH ENERGY PHYSICS BUDGET REQUEST

The FY 1989 request for the research portion of High Energy Physics is $220.4 million. Of the total amount, $144.9 million is for Operating Expenses, $66.1 million is for Capital Equipment and $9.4 million is for Construction (Tables 3 & 4). This request falls under the General Science and Research appropriation. I will discuss the Basic Research User Facilities request for High Energy Physics and for the Superconducting Super Collider after I describe the budget request for our High Energy Physics research program.

The Operating Expenses request supports university and laboratory research groups who carry out experimental and theoretical research in high energy physics as well as detector R&D studies to develop new and advanced concepts and technologies. FY 1988 will be a year of strong research output from the new Tevatron and SLC colliders and new Tevatron fixed target capabilities.

Table 3
General Science and Research
High Energy Physics
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Physics Research	$109.0	$116.7	$123.8
High Energy Technology	21.4	20.3	21.1
Subtotal	$130.4	$137.0	$144.9
Capital Equipment	59.0	63.8	66.1
Construction	12.5	16.6	9.4
Total High Energy Physics	$201.9	$217.4	$220.4

Table 4
General Science and Research
High Energy Physics
Budget Authority
($ in millions)

Construction

Project	TEC	FY 1988	FY 1989 Request
Central Computing Facility (86-R-104)	$24.1	$ 11.0	$ 3.6
General Plant Projects (GP-E-103)	5.8		5.8
General Plant Projects (GP-E-103)	5.6	5.6	-
Total Construction		$ 16.6	$ 9.4

Research activity planned for FY 1989 is expected to be very productive as the data collected in FY 1988 are analyzed and these new facilities operate in an intensive data-taking mode.

The FY 1989 High Energy Physics Capital Equipment request of $66.1 million will provide the particle detection apparatus and data analysis systems essential to forefront high energy physics research. In FY 1989, priority will be given to continued progress on the fabrication of the SLAC Large Detector for the Stanford Linear Collider and the D-Zero Detector for the Tevatron collider. With this continued progress, first physics research with these detectors is planned to be started early in FY 1990 and the detectors can be completed by the end of FY 1990.

The FY 1989 Construction request is $9.4 million. This request includes final funding for the central computing upgrade at Fermilab ($3.6 million) which is scheduled to be completed in 1989. This upgraded computing capability is required to analyze the large volume of new physics data coming from the Tevatron program. This request also provides $5.8 million for General Plant Projects at Brookhaven National Laboratory which are required to keep the general plant (e.g., buildings, roads and utilities) in good operating condition.

BASIC RESEARCH USER FACILITIES--HIGH ENERGY PHYSICS (BRUF-HEP)

The ability to carry out forefront exploratory research in high energy physics is critically dependent on the effective operation of the major accelerator and collider facilities. The new BRUF appropriation provides funds to build, operate, and maintain these facilities. The total BRUF-HEP request for FY 1989 is $346.4 million. Of that total amount, $301.7 million is for Operating Expenses. The Capital Equipment request is for $13.7 million; and the Construction request is $31.0 million (Tables 5 & 6).

Within the Operating Expenses request, the highest priority will be given to operating the new world forefront research capabilities of the Stanford Linear Collider and the Tevatron proton-antiproton collider and fixed target programs. Operation at SLAC of the upgraded Positron-Electron Project collider with the improved Time Projection Chamber detector and operation at BNL of the Alternating Gradient Synchrotron program on rare kaon decay will also be very important. Accelerator R&D, including studies of advanced accelerator concepts, will be supported at about the same level of effort as in FY 1988.

The Capital Equipment request of $13.7 million is needed to maintain and enhance the scientific capability of the accelerators and colliders and to provide the secondary beam line components and portable shielding needed to do high quality high energy physics experiments. Funding in FY 1989 will provide for support of planned accelerator maintenance, upgrading and modifications at Fermilab, SLAC and BNL.

Table 5
Basic Research User Facilities (BRUF)
High Energy Physics
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Facility Operations	$204.3	$233.3	$256.6
High Energy Technology	39.1	43.4	45.1
Subtotal	$243.4	$276.7	$301.7
Capital Equipment	$ 11.1	12.9	13.7
Construction	18.9	24.6	31.0
Total BRUF-HEP	$273.4	$314.2	$346.4

Table 6
Basic Research User Facilities (BRUF)
High Energy Physics
Budget Authority
($ in millions)

Construction

Project	TEC	Prior Year	FY 1988	FY 1989 Request
Accelerator Improvements and Modifications (89-R-301)	$11.7	-	$ -	$ 11.7
Accelerator Improvements and Modifications (88-R-101)	11.2	-	11.2	-
General Plant Projects (GP-E-302)	5.3	-	-	5.3
General Plant Projects (GP-E-103)	5.1	-	5.1	-
AGS Accumulator/Booster (86-R-105)	31.7	$4.4	8.3	$ 14.0
Total Construction		$4.4	$ 24.6	$ 31.0

Of the $31.0 million Construction request, $14.0 million will provide for continued construction of the AGS Accumulator/Booster at BNL. This project, which is scheduled for completion in FY 1991, will improve the performance and capabilities of the AGS and will significantly increase the data collection rate for the experimental program already planned and underway. The Construction request also includes funds for a constant level of effort for Accelerator Improvements and Modifications Projects ($11.7 million) that are required to maintain the scientific effectiveness and the operating reliability and efficiency of the accelerator and colliding beam facilities. Finally, the request provides $5.3 million for a constant level of effort for General Plant Projects at Fermilab and SLAC that are required to keep the general plant of the laboratories (e.g., buildings, roads and utilities) in good operating condition.

BASIC RESEARCH USER FACILITIES--SUPERCONDUCTING SUPER COLLIDER (BRUF-SSC)

As I noted earlier, experimental research in high energy physics requires the use of large particle accelerators, colliding beam devices and detectors. To extend the research frontier into a region never before studied and where major breakthroughs in understanding are firmly anticipated, larger, more powerful accelerators are required. Major upgrades of the Stanford Linear Collider at SLAC and the Tevatron Collider at Fermilab will keep the U.S. High Energy Physics program internationally competitive and at the cutting edge of physics research for the next several years. But even these facilities will not reach high enough energies to allow physicists to investigate the region

where major breakthroughs are anticipated. There are many important unanswered questions such as whether there are even more fundamental particles than quarks and leptons, what determines their basic properties and whether there are new, undiscovered forces of nature. To advance the research frontier, a new, more powerful accelerator is needed. The proposed Superconducting Super Collider is that facility.

As Secretary Herrington has noted, this Presidential initiative is a watershed for America's scientific and technological leadership and another clear sign that this Administration is committed to keeping this Nation on the cutting edge of world leadership and competitiveness.

The SSC will be the world's preeminent facility for high energy physics research. It will be a 20 trillion electron volt (TeV) on 20 TeV proton-proton collider, at the heart of which will be two rings containing about 10,000 superconducting magnets located in a racetrack-shaped tunnel that will be about 10 feet in diameter and 53 miles long. These magnets will focus and guide two beams of protons in opposite directions around the tunnel. The collisions that result when the two particle beams are boosted to nearly the speed of light and allowed to collide head on at four interaction regions will create many different kinds of particles that will be detected and analyzed.

The SSC's energy level of 20 TeV per beam will be 20 times higher than the energy of the Tevatron collider in Illinois, the highest energy accelerator in the world. With this higher energy—which is similar to greater magnification

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power in a microscope--scientists will be able to explore the basic structure of matter and the nature of the fundamental forces far beyond the horizon of current knowledge.

Each mystery unravelled along this path of research has dramatically altered the way we live, though often the consequences are not apparent for many years. Man's first steps on the moon would have been impossible without the basic conceptual understanding of gravity developed centuries earlier by Galileo and Newton. Radio and television and all the other electronic wonders of our age rest on the understanding of electricity and magnetism developed during the last century by Faraday, Maxwell and Hertz. Nuclear power and nuclear medicine can be traced to the quests in the first half of this century by Rutherford, Bohr, and Fermi for a deeper understanding of the atom and the nucleus.

Today, high energy physicists continue to be pioneers in the search for fundamental new knowledge. The frontiers they explore relate to those of other fields, especially astrophysics and nuclear physics, and advances in one field often benefit the others. The tools that have been developed-- high energy particle accelerators and particle detectors--have been used to enrich other sciences as well. Materials scientists, chemists, and biologists routinely use synchrotron light sources that depend on accelerators originally developed for high energy physics research. There are many other examples of applications of technology flowing from high energy physics in the areas of nuclear medicine, electronics, computer chips, superconductivity, and cryogenics. For example, it has been estimated that many millions of

Americans each year receive medical diagnosis or treatment benefits from nuclear medicine techniques which were developed from the technologies and particle accelerators used in basic physics research. Striking examples include computer-assisted tomography (CAT), positron emission tomography (PET), and magnetic resonance imaging (MRI) scans--the latter of which is estimated to be a multi-million dollar industry.

Although we cannot predict what applications will derive from SSC research or when they will happen, we can be confident that further advances in basic knowledge will contribute to America's economic and technological competitiveness through applications of the new knowledge gained and the discoveries made, and through technological spin-offs similar to those we have seen up to now.

Another important benefit of the SSC is that it will be a valuable training ground for American scientists and will attract many of the world's leading physicists to live and work in the United States. The SSC would be utilized by over 100 American university-based research teams, thereby continuing the precedent of accelerator facilities as a career launching pad for young scientists and engineers. The SSC would be a viable inspiration for young people to pursue careers in advanced science and technology by challenging a new generation to unlock the secrets of nature.

The SSC is essential if we are to maintain leadership in high energy physics research into the 21st century--with all of the many benefits I have just discussed. This is a field in which our country has excelled. In the past,

U.S. leadership has been maintained by successive commitments to more powerful, sophisticated accelerators. The Super Collider represents the next generation of advanced accelerators. The technology to be used in the SSC is similar in principle to that of earlier, smaller facilities; but this technology will be advanced to higher levels of performance to achieve the most cost-effective and reliable facility possible. The technology developed in building the SSC and its experimental equipment is expected to lead to industrial innovation in many areas including superconducting magnets, cryogenics, computing and electronics. In this age of keen international competition, the SSC is vital to the United States' ability to maintain its leadership in the laboratory and the marketplace.

FY 1989 represents the first year of substantial funding for the Super Collider. As such, it represents only one part of a very broad Administration effort to advance in all areas of science rather than focusing on one area to the detriment of all others. It is worth noting that, in the five-year period between FY 1988 and FY 1993, excluding funding for the SSC, there is still a projected increase in DOE's other basic science research activities of over 34 percent. This is an average annual increase of over 7 percent. Clearly, ER programs have not been affected by the SSC. In this same period, the budget for the National Science Foundation is projected to increase by almost 76 percent; and the budget for the National Aeronautics and Space Administration is scheduled to increase by 67 percent. This latter increase reflects another Administration initiative-the space station. These numbers clearly show that the SSC is not being funded at the expense of other

science. As I noted earlier, this approach recognizes the very substantial role that scientific research plays in our economy and in enhancing the quality of our lives.

The Department is proceeding on course with its plan to select the site for the SSC in January 1989. As you know, the Department announced its list of seven best qualified sites in January. Detailed analysis of these sites is now in progress. In addition, the Department is actively seeking international collaboration and cost-sharing with other countries.

The FY 1989 request includes $363.0 million for the Superconducting Super Collider (Table 7). The total project cost of the SSC is estimated to be $4.4 billion in FY 1988 dollars, including the costs of construction, R&D, detectors, computers and preoperations. In escalated dollars the total project cost is $5.3 billion. With an FY 1989 project start, the target completion date is 1996.

The SSC request includes $64.0 million in Operating Expenses. This will permit R&D on SSC technical systems to proceed at a substantially enhanced level in FY 1989. The work will focus on optimizing the design of the magnets and on industrialization of the magnet fabrication process.

In FY 1989 the SSC will have its first significant capital equipment requirements. The $16.0 million Capital Equipment request will provide equipment required to support the magnet R&D program for testing and analysis of magnets and for testing various other technical components and systems.

Table 7
Basic Research User Facilities
Superconducting Super Collider (SSC)
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses	$ 20.0	$ 25.0	$ 64.0
Capital Equipment	-	-	16.0
Construction	-	-	283.0
Total SSC	$ 20.0	$ 25.0	$363.0

The requested funds will provide for such equipment as power supply systems; a large helium refrigerator; test, control and measurement equipment; and components for detector development and prototyping.

The Construction request of $283.0 million will allow initial construction activities to proceed on a broad front in FY 1989. The request includes $39.0 million for detailed engineering design of technical systems and conventional facilities, $60.0 million for long-lead items for power stations and utilities distribution systems, $27.0 million for site preparation, $55.0 million for injector technical components which are early critical path items, $65.0 million for long lead procurements for collider components (including magnet tooling, superconducting materials and magnet steel) and $37.0 million for project management, support equipment and rental space.

In summary, I believe that the SSC demonstrates clearly the commitment of President Reagan and his Administration to pursue new initiatives in science and technology and, in doing so, to enhance the economic and technological health of the United States.

NUCLEAR PHYSICS

The goal of the Nuclear Physics program is to understand the structure and interactions of atomic nuclei. This understanding is essential to unraveling the mysteries of the world around us because nuclear processes determine the

physical characteristics of our universe. For this very reason, advances in nuclear physics have both short and long-term effects well beyond the immediate boundaries of the field.

The results of nuclear physics research have made many contributions to our society and economy. For example, every hospital contains numerous examples of nuclear science at work. Especially prominent is the use of radioactivity and nuclear beams for diagnosis and therapeutic treatment of cancer. Nuclear energy makes a substantial contribution to the Nation's energy supply; the smoke detectors we have in our homes contain radionuclides; nuclear techniques in the form of ion implantation have revolutionized the semiconductor industry; and nuclear techniques for geophysical exploration, testing of materials, and archeological dating and siting are widely used.

The Nuclear Physics program continues to increase our knowledge of atomic nuclei as well as our capability to expand that knowledge base. Scientists now know that neutrons and protons, the constituents of the nucleus that are called nucleons, are composed of smaller constituents called quarks. These quarks are bound together in tightly knit groups of threes. However, scientists have not been able to isolate quarks outside the nucleus. It is as if quarks were tied together by elastic strings that cannot be broken. This confining mechanism forms the basis of the newest theory of the strong nuclear force, quantum chromodynamics.

The total Nuclear Physics program develops and supports a wide range of large and complex accelerator facilities which provide the beams of particles upon which the experiments are based. The diverse aspects of nuclear experimental research are addressed by using beams of light ions, heavy ions, electrons and protons over a wide range of particle energies. The collision of these beams with targets of atomic nuclei is the primary means by which scientists learn about the structure and forces acting within nuclei.

Among the program's accomplishments in 1987, the first gamma ray beam from the Laser-Electron-Gamma-Source (LEGS) at Brookhaven National Laboratory was produced. This unique facility produces gamma rays at a well-defined energy by bouncing ultraviolet light produced in a laser off the electrons in the circulating beam at Brookhaven's National Synchrotron Light Source (NSLS). The scattered light emerges with a much higher energy than it started with (thus becoming gamma rays with an energy of 300 million electron volts) as a result of the collisions with the electrons. By detecting the electrons from the NSLS beam which have been hit by the light, scientists can select a beam of gamma rays of the appropriate energy for use in experiments. LEGS is unmatched in the intensity and purity of the gamma ray beam it will provide for research. In addition, the gamma rays are 100 percent polarized, adding the unique aspects of spin to the photo-nuclear experiments planned for the new facility.

Also in 1987, Argonne's superconducting ATLAS system reached the limits of its current ion source and injector by accelerating ions with atomic masses of more than 120. ATLAS was the world's first working accelerator to use

superconducting radio frequency cavities and has become a prototype for many other facilities throughout the world. A new positive-ion injector is under development that eventually will enable ATLAS to accelerate ions as massive as uranium, which is the heaviest stable element. In addition, a new recoil mass spectrometer is being installed to make most effective use of the new beams.

In addition to accelerator facilities, the Nuclear Physics and High Energy Physics programs support "nonaccelerator" experiments such as those to measure the lifetimes of protons and radioactive nuclei, to search for exotic particles and unusual modes of decay, or to open new windows on the universe through information provided by neutrino detectors. Using such detectors, physicists supported by the High Energy and Nuclear Physics program gathered important information about one of 1987's most spectacular scientific events. The brightest exploding star in the last 363 years, called a supernova, was observed several hours before its visible light reached the earth by one detector located deep in a salt mine under Lake Erie and a second which was deeply buried in the mountains of Japan. The detectors recorded a burst of neutrinos, which is the first time that neutrinos from outside our solar system have been observed. Neutrinos are the most abundant particles in the universe, but scientists know relatively little about their properties. Observation of the 1987 supernova not only provided information on the stability and mass of neutrinos, but also provided a key to the dynamics of the great explosion which marked the end of this star's life.

FY 1989 NUCLEAR PHYSICS BUDGET REQUEST

The FY 1989 budget request for the research portion of the Nuclear Physics program is $141.4 million. Of that total, $118.1 million is for Operating Expenses, $17.5 million is for Capital Equipment and $5.8 million is for Construction (Tables 8 & 9). I will discuss the Basic Research User Facilities request for Nuclear Physics after describing the Nuclear Physics research program request.

The FY 1989 Operating Expenses request will provide for continued experimental research on the basic structures and characteristics of nuclei and for the investigation of quarks within nuclei and of the fundamental forces of nature acting within extended nuclear matter. Research in low and medium energy nuclear physics, in heavy ion nuclear physics and in nuclear theory will continue at approximately the FY 1988 level. This research will focus on current problems of high scientific and technological interest and will continue to prepare the program to exploit major new facilities. Funding is also devoted to maintaining the effective operation of the upgraded accelerators at the University of Washington and Yale University, of the superconducting cyclotron at Texas A&M constructed with State and private funds, and of the Tandem/AGS high energy heavy ion facility at Brookhaven National Laboratory.

Operating funds also will support continued vigorous R&D aimed at providing a solid scientific and technical base for the planned relativistic heavy ion collider, one of four facilities identified by the scientific community as

Table 8
General Science and Research
Nuclear Physics
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Medium Energy Nuclear Physics	$ 32.7	$ 35.5	$ 37.5
Heavy Ion Nuclear Physics	36.4	40.9	43.3
Low Energy Nuclear Physics	24.0	26.2	26.3
Nuclear Theroy	10.0	10.5	11.0
Subtotal	$103.1	$113.1	$118.1
Capital Equipment	$ 14.8	$ 16.6	$ 17.5
Construction	4.3	5.2	5.8
Total Nuclear Physics	$122.2	$134.9	$141.4

Table 9
General Science and Research
Nuclear Physics
Budget Authority
($ in millions)

Construction

Project	TEC	FY 1988	FY 1989 Request
Accelerator Improvements and Modifications (89-R-201)	$ 2.6	-	$ 2.6
Accelerator Improvements and Modifications (88-R-201)	2.3	$ 2.3	-
General Plant Projects (GP-E-300)	3.2	-	3.2
General Plant Projects (GP-E-300)	2.9	2.9	-
Total Construction		$ 5.2	$ 5.8

being critical to the future needs of the Department's basic research programs. Special emphasis will be made to accommodate training of young scientists and to ensure that the nuclear theory program accommodates the latest scientific priorities.

The FY 1989 Capital Equipment request of $17.5 million is needed to provide for particle detection systems, data acquisition and analysis systems and instrumentation to improve the performance of Nuclear Physics accelerators. These funds are essential for effective use of the accelerator and detector facilities operated by the Nuclear Physics program. The FY 1989 request will maintain the overall level of effort and will provide for general purpose equipment to meet the laboratory-wide needs of Lawrence Berkeley Laboratory.

The Construction request of $5.8 million includes $2.6 million for Accelerator Improvements and Modifications (AIM) and $3.2 million for General Plant Projects (GPP). These projects are needed to maintain the scientific effectiveness, reliability and efficiency of Nuclear Physics facilities. GPP projects focus on general laboratory facilities while AIM projects address the need for improvements to research accelerators and related experimental facilities.

BASIC RESEARCH USER FACILITIES--NUCLEAR PHYSICS (BRUF-NP)

The Nuclear Physics program requires the effective operation and maintenance of the accelerator facilities which provide the beams of particles upon which the experiments are based. The BRUF-NP program operates and maintains the

following scientific user facilities: the Clinton P. Anderson Meson Physics Facility (LAMPF) at Los Alamos National Laboratory, the Bevalac at Lawrence Berkeley Laboratory, and the Tandem/AGS at Brookhaven National Laboratory. This program also funds construction of the major new facilities required to address the most important scientific questions. To meet this objective, construction of the Continuous Electron Beam Accelerator Facility (CEBAF) project was initiated in FY 1987.

The FY 1989 BRUF-NP request is for $122.7 million. Of that amount, $75.0 million is for Operating Expenses; $1.0 million for Capital Equipment and $46.7 million is for Construction (Tables 10 & 11).

The Operating Expenses request will support medium energy physics operations and facility maintenance at approximately the FY 1988 level of activity. The $49.6 million requested for this subprogram will support the operation of LAMPF as well as the light ion operation of the Bevalac. R&D activities required for the construction of CEBAF and preparation for operation of the laboratory are also carried out under the medium energy subprogram. The operation of heavy ion nuclear physics facilities at BNL and LBL will be supported at approximately the FY 1988 level of activity.

The $1.0 million Capital Equipment request is needed to provide for instrumentation to improve and maintain performance of Nuclear Physics accelerators at BNL, LBL and LANL.

Table 10
Basic Research User Facilities (BRUF)
Nuclear Physics
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Medium Energy Nuclear Physics	$ 47.9	$ 48.5	$ 49.6
Heavy Ion Nuclear Physics	23.9	24.3	25.4
Subtotal	$ 71.8	$ 72.8	$ 75.0
Capital Equipment	$ 1.1	$ 1.1	$ 1.0
Construction	19.8	36.3	46.7
Total BRUF-NP	$ 92.7	$110.2	$122.7

Table 11
Basic Research User Facilities (BRUF)
Nuclear Physics
Budget Authority
($ in millions)

Project	Construction TEC	Prior Year	FY 1988	FY 1989 Request
Accelerator Improvements and Modifications (89-R-501)	$ 1.7	-	-	$ 1.7
Accelerator Improvements and Modifications (88-R-201)	2.1	-	$ 2.1	-
Continuous Electron Beam Accelerator Facility (87-R-203)	265.0	$ 16.2	33.5	44.5
General Plant Projects (GP-E-500)	0.5	-	-	0.5
General Plant Projects (GP-E-300)	0.7	-	0.7	-
Total Construction		$ 16.2	$ 36.3	$ 46.7

Of the total Construction request of $46.7 million, $44.5 million is for continued construction of CEBAF. The main research tool at CEBAF will be an accelerator with electron beams in the energy range of 0.5 to 4.0 GeV. Its combination of high-energy, high-current, and continuous beam with excellent beam quality is expected to revolutionize the production and collection of data for nuclear physics research. When CEBAF's energy is at its lowest, the electron beam will interact with the protons and neutrons that are the building blocks of nuclei. When its energy is at the high range, the electron beam from the accelerator will probe the underlying quark structures of protons or neutrons, providing a direct test of the newest concepts of nuclear matter, including quantum chromodynamics. Thus, the facility will provide the world's first capability to access the critical transition region between the traditional and emerging descriptions of nuclear matter.

The $44.5 million in FY 1989 Construction funds for CEBAF will be used for major hardware procurement of radiofrequency cavities and the central helium liquifier. Construction of the beam enclosure tunnel and support structures will continue. In addition, initial procurement of beam control apparatus and prototype power supplies will take place. Progress in FY 1988 includes complete preparation of the site, initial construction of the beam enclosure tunnel, procurement and testing of prototype superconducting accelerator elements, rehabilitation of an existing laboratory building, and approved architectural designs for the beam enclosure and office building. The remainder of the Construction request will support Accelerator Improvements and Modifications ($1.7 million) and General Plant Projects ($.5 million) at the existing facilities.

GENERAL SCIENCE PROGRAM DIRECTION

The FY 1989 budget request for General Science Program Direction is $3.2 million. This request is under the General Science and Research appropriation. The requested funds are required to provide for the salaries, benefits, travel and other expenses associated with 46 full-time equivalents in the Office of High Energy and Nuclear Physics and associated support staff required to administer these programs.

Now let me turn to the Basic Energy Sciences program.

BASIC ENERGY SCIENCES

Basic research is the first link in the chain of events from scientific discovery to economic growth through technological innovation. Results from research sponsored by the Basic Energy Sciences (BES) program become an integral part of the information base used in the development of the applied technologies within the Department and are equally important within the private sector. In addition to underpinning technology development, BES-supported research activities train our future scientists and help us attain our national goals--better health and quality of life, economic strength, energy self-sufficiency, national security--goals calling for continuing and significant technological advances.

The BES program annually supports approximately 1400 research projects at over 200 separate institutions with direct support for about 4000 investigators in the physical, biological, and mathematical sciences. These projects are selected on the basis of scientific excellence, relevance to DOE long-term goals, and their contribution toward a program responsive to the needs of the Nation. BES also provides funding for the supercomputer centers used for computation by the High Energy Physics, Nuclear Physics, and Biological and Environmental Research programs as well as for its own program.

The Basic Energy Sciences program undertakes research primarily at national laboratories and universities and, to a lesser extent, at private research laboratories and other government laboratories. The majority of BES-supported research is carried out at national laboratories. These laboratories are especially valuable in conducting basic research because researchers there also are involved in many aspects of the applied energy programs, thus facilitating transfer of scientific information and technology from experiment to application. In addition, the stability of the organization and specialized capabilities which exist at the laboratories in many instances are unmatched. The BES program also provides significant support to universities both directly and through the participation of university faculty members, postdoctoral students or graduate students in research at the national laboratories and at user facilities.

In addition to universities and national laboratories, BES supports research in and maintains strong ties with industry. Representatives from different industries serve on the BES Advisory Committee; experts from industry

participate in the review of research proposals and use the specialized facilities sponsored by BES; industrial scientists participate in advisory committees to the national laboratories; and industry representatives are invited to attend BES conferences and workshops on special topics.

No summary of research results for such a diverse program can ever be complete or even fully representative of the program. Since the BES program covers such a wide spectrum of research, I can highlight only a few accomplishments in this statement. I would be happy to supply the Committee with additional representative accomplishments for each of our research areas.

Of special note, a BES project at UCLA has been recognized by a share of the 1987 Nobel Prize in Chemistry. The Prize was awarded to Professor Donald J. Cram of UCLA, Dr. Charles J. Pedersen of DuPont and Professor Jean-Marie Lehn of the Louis Pasteur University for developing new chemical methods for recovering scarce metals and purifying common metals using much less energy than in the past. Professor Cram's research program is providing new insight into the selective binding of materials which can "tie up" a valuable metal in the presence of other common ones and allow the new chemical combination to be extracted. The novel substances prepared by Cram's research team can bind molecules as well as metals and can be designed to mimic the highly specific activity of enzymes, the remarkable catalysts that are essential to all forms of life. Thus, in addition to the applicability of Cram's work to recovery of scarce metals and purification of waste streams of toxic or radioactive metals, the principles he has discovered also have widespread implications for the understanding of many processes that occur in nature.

In the materials sciences area, theoretical modeling of materials phenomena provides a basis for predicting the behavior of materials and can be very valuable in supporting experimental results and uncovering new phenomena. Recent application of a newly developed theoretical technique has resulted in the calculation of stress levels that cause brittle cracks to grow in materials. This knowledge will help us to determine safe stress levels without the time consuming process of testing every possible composition and structure.

An important "first," the demonstration of the x-ray laser, received one of the 1987 IR-100 awards which are presented annually by Research & Development magazine to the 100 most significant new technical products in the world. When fully developed for microscopy purposes, laser-based x-ray microscopy offers, for the first time, the prospect of observing the insides of living cells, including the distribution of chemical elements within a cell. The first microscopic images using the x-ray laser were recently obtained but the wavelength of the laser is still too long to image the cells; further work is in progress.

Last year was also an important year in the development of high-temperature superconductors. Although superconductivity was discovered in 1911, applications to date have depended on cooling with liquid helium. At the end of 1986, a breakthrough occurred in the temperature limit and newly developed "high-temperature" materials have now been found which superconduct above the temperature of liquid nitrogen which is considerably cheaper and easier to use

than liquid helium. To take advantage of the discovery of the new class of high-temperature superconductors, the Office of Basic Energy Sciences has supported research aimed at addressing scientific and technical obstacles that must be overcome to use the new materials in technologically important systems. The BES research program is addressing fundamental questions regarding the mechanism by which the new high-temperature superconductor materials become superconducting, the key to finding even better new materials. The program also is addressing the synthesis of new materials to expand the range of these parameters.

The national laboratory structure and the availability of BES's major user facilities have been critically important to the ability of the BES program to quickly redirect research into this important area. Although the funding request for these facilities is now under the Basic Research User Facilities appropriation which is discussed later in my statement, I would like to discuss some of the contributions being made by BES research to our ever-growing understanding of high-temperature superconductors.

Within a few weeks after the discovery of the new higher temperature superconductors, scientists at Argonne National Laboratory's Intense Pulsed Neutron Source carried out neutron scattering studies of the yttrium-barium-copper oxide material and produced definitive results on its structure. This new information on the exact arrangement of the atoms in the compound has provided the basis for understanding all of the subsequent experiments conducted and theories developed to understand why these compounds are superconducting at such high temperatures.

At the High Flux Beam Reactor (HFBR), scientists are learning more about two phenomena that play a key role in high-temperature superconducting materials, atomic vibrations and the properties of electrons within these materials. Neutron scattering studies of atomic vibrations have confirmed scientists' belief that the behavior of these materials is similar to conventional low-temperature superconducting materials. However, other neutron scattering research at HFBR led to unexpected behavior related to electron properties giving rise to magnetism. Specifically, in the pure lanthanum-copper oxide material, the copper oxide sheets exhibit a novel type of magnetic order at quite high temperatures. Thus, from the neutron scattering results, one normal and one unanticipated, a number of questions still remain that have to be explored further.

Synchrotron radiation also is useful to study electron properties in materials. By illuminating the superconductors with light from Brookhaven's National Synchrotron Light Source (NSLS), it was possible to excite the electrons from the different atoms and determine their energies or velocities within the material. Studies of the yttrium-barium-copper oxide superconductor showed that the electrons do not have energies as high as the calculations would suggest and that only a small number of electrons have the highest energy. This latter observation is contrary to what had normally been anticipated for superconductors. This work was one of the first reported photoemission studies of the high-temperature superconductors and the unusual results have now been confirmed by other groups.

FY 1989 BASIC ENERGY SCIENCES BUDGET REQUEST

The FY 1989 budget request for the research portion of the BES program is $375.6 million. Of the total, $344.4 million is for Operating Expenses; $25.6 million is for Capital Equipment; and $5.6 million is for Construction (Tables 12 & 13). The overall increase in funding for the Basic Energy Sciences program is 3 percent if FY 1988 Congressionally initiated projects are excluded. This request falls under the Energy Supply R&D appropriation. I will discuss the Basic Research User Facilities request for BES separately.

The Operating Expenses request will allow us to conduct research aimed at uncovering new energy-related information in materials sciences, chemical sciences, applied mathematical sciences, engineering and geosciences, energy biosciences and advanced energy projects. Specifically, the FY 1989 budget will provide the necessary resources to maintain the existing core program in each of the subprograms in BES and to permit initiation of a limited number of new research projects in those areas that have the highest scientific merit.

No two areas of scientific research illustrate better the stakes, promises, and consequences of research funding better than semiconductors and superconductors, which are funded in our Materials Sciences subprogram. Each holds out the hope of new technological advances. Future developments in computers, transportation, energy, defense, and medicine will be affected by the speed at which advances take place in these technologies.

Table 12
Energy Supply R&D
Basic Energy Sciences
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Materials Sciences	$118.6	$126.0	$134.6
Chemical Sciences	79.2	88.2	93.1
Applied Mathematical Sciences	38.3	42.4	43.0
Engineering and Geosciences	29.1	31.4	33.4
Advanced Energy Projects	12.6	14.3	14.6
Energy Biosciences	16.3	20.2	20.8
Program Direction	4.1	4.5	4.9
Subtotal	$298.2	$327.0	$344.4
Capital Equipment	24.6	25.0	25.6
Construction	109.4	85.1	5.6
Total Basic Energy Sciences	$432.2	$437.2	$375.6

Table 13
Energy Supply Research and Development
Basic Energy Sciences
Budget Authority
($ in millions)

Construction

Project	TEC	FY 1988	FY 1989 Request
Accelerator Improvement Projects (89-R-401)	$ 0.7	-	$ 0.7
General Plant Projects (89-R-400)	4.9	-	4.9
General Plant Projects (88-R-400)	4.9	$ 4.9	-
Center for Advanced Materials (84-ER-112)	40.2	7.6	-
Congressionally Initiated University Projects		72.6	-
		$85.1	$ 5.6

The discoveries and advances in these areas have the entire scientific community excited. In high-temperature superconductivity, the Operating Expenses request will continue to fund research on methods for preparing superconducting materials with better properties, such as higher critical currents. Newer materials are also being investigated to understand the structure and mechanism of high-temperature superconductivity. The FY 1989 request includes $16.7 million, an increase of $1.5 million, for high-temperature superconducting research in the BES program.

The Department has played an important role in the development of the U.S. semiconductor technology base and continues to support research in this area. Semiconductors are materials that form the base for all solid state microelectronics circuitry. In FY 1989, semiconductor research supported by the Basic Energy Sciences program will continue to emphasize the science related to compound semiconductors and to provide synchrotron radiation useful to the Nation's researchers in studies of semiconductors. The FY 1989 level of funding in research related to semiconductors is planned to be about the same as in FY 1988, approximately $7.0 million.

Continued support will also be provided in FY 1989 for basic combustion research which will be important for providing advances in coal technology, engines, and for handling the spectrum of environmental problems in any combustion system. The level of funding in FY 1989 will be $15.1 million, an increase of $0.3 million. Scientists are uncovering new knowledge about the chemistry and kinetics of combustion that ultimately will enable automobile companies and utilities to improve operating systems.

The FY 1989 request for Program Direction is $4.9 million. These funds are required to pay for the personnel costs of 63 full-time equivalents to direct, manage, and support BES's multidisciplinary research efforts.

The Capital Equipment request for FY 1989 is $25.6 million. These funds will ensure that BES-supported researchers will continue to have the equipment needed to conduct advanced research, much of which involves experiments at extremes of temperature and pressure. Reliable, precise measurements under these conditions challenge the current state-of-the-art. Replacements and new equipment funded under this request, which are important to the continued success of the program, include such items as electron microscopes, neutron spectrometers, molecular beam equipment and computers for equipment control and data analysis.

The Construction request for FY 1989 is $5.6 million. This request includes continued funding for General Plant Projects and Accelerator Improvement Projects. Each of these categories is comprised of several smaller projects which are necessary to maintain plant and facilities at several DOE laboratories. The projects include a wide spectrum of improvements from roads and buildings to safety and security upgrades necessary to protect property and personnel and to modify existing scientific facilities to allow the most cost-effective utilization of their technical capabilities.

BASIC RESEARCH USER FACILITIES - BASIC ENERGY SCIENCES (BRUF-BES)

The remaining part of the Basic Energy Sciences program which I would like to discuss falls under the Basic Research User Facilities (BRUF) appropriation. Within the Basic Energy Sciences program, seven existing major user facilities are included in this new appropriation together with R&D, capital equipment and construction funding for two new facilities under construction. The seven existing facilities are: the High Flux Beam Reactor and National Synchrotron Light Source at Brookhaven National Laboratory, the Combustion Research Facility at Sandia-Livermore, the Stanford Synchrotron Radiation Laboratory (SSRL) at Stanford University, the Intense Pulsed Neutron Source at Argonne National Laboratory, the Los Alamos Neutron Scattering Center at Los Alamos National Laboratory, and the High Flux Isotope Reactor (HFIR) and the Transuranium Processing Plant at Oak Ridge National Laboratory. HFIR is currently being refurbished, and we expect it to restart later this year. One of the new facilities, the 1-2 GeV Synchrotron Radiation Source at Lawrence Berkeley Laboratory is already under construction, and funding is being requested to initiate construction of a second new facility, the 6-7 GeV Synchrotron Radiation Source at Argonne National Laboratory. Each of these seven facilities is unique in the capabilities which it offers the scientific and engineering community; and the success and promise of these facilities in meeting the Nation's research needs has been praised by many high-level committees.

Earlier, I mentioned some of the many accomplishments, primarily in the area of superconductivity, made possible by the existence of these facilities which are the key to research progress in many fields, including the physical, biological and mathematical sciences. Their availability to a wide range of researchers from academia, industry, and other government laboratories makes them especially valuable to the Nation. To gain a better understanding of their scientific and, ultimately, economic importance one only has to look at the community of users involved in research at the National Synchrotron Light Source (NSLS). Table 14 shows some of the many university, industry and government organizations that take advantage of the research capabilities at NSLS.

In addition to allowing many non-DOE researchers to carry out research directly at these facilities, some of these facilities produce research materials that are made available to other organizations. For example, the High Flux Isotope Reactor (HFIR) and Transuranium Processing Plant at ORNL have produced two isotopes used in cancer therapy research supported by the National Cancer Institute. Einsteinium-253 and fermium-255, which emit alpha particles with short half lives, are bound to monoclonal antibodies which are able to direct these radionuclides and cytotoxic drugs to malignant sites. Alpha emitters are ideal for cancer therapy because their entire radiation energy is confined to a very small range. Therefore, the number of alpha "strikes" required to kill a cancer cell is much smaller than for other types of radiation. It is also important that alpha emitters to be used in cancer therapy have relatively short half-lives so that the radiation dose to the patient can be better controlled.

Table 14
Basic Energy Sciences
Basic Research User Facilities
National Synchrotron Light Source
(Sample of Users)

Universities	Industry	Government
University of California	AT&T	National Bureau of Standards
Georgia Institute of Technology	Exxon	National Institutes of Health
Georgia State University	IBM	Naval Research Laboratory
Harvard University	Mobil	Veterans Administration
University of Illinois	Celanese	Lawrence Livermore National Laboratory
Louisiana State University	Allied Signal	Los Alamos National Laboratory
Massachusetts Institute of Technology	Union Carbide	Sandia National Laboratories
Michigan State University	3M	
University of New Orleans	Boeing	
University of North Carolina	Chevron	
Northwestern University	Smith, Klein and French	
University of Oregon	SRI	
University of Pennsylvania	Xerox	
Princeton University		
University of South Carolina		
Stanford University		
University of Texas		

The FY 1989 budget request for BRUF-BES is $140.5 million. Of that total, $86.3 million is for Operating Expenses; $6.1 million is for Capital Equipment; and $48.1 million is for Construction (Tables 15 & 16).

The BRUF-BES Operating Expenses request of $86.3 million provides for continuing the operation of the seven existing facilities funded in this account, including HFIR which, as I already indicated, is in a stand down condition but being readied for restart. Within the Operating Expenses request, $8.2 million is necessary to continue R&D activities associated with the 1-2 GeV Synchrotron Radiation Source at LBL and the 6-7 GeV Synchrotron Radiation Source at ANL. The R&D funds for the former are focused on improvements to the magnet lattice and related activities. For the 6-7 GeV Source, research activities will focus on prototypes of the dipole magnets and insertion devices.

Reliable, precise measurements at the seven BES facilities challenge the current state-of-the-art; and improvements and replacement hardware are necessary in a timely fashion. The Capital Equipment request of $6.1 million provides for high priority synchrotron radiation equipment and neutron scattering equipment.

Of the $48.1 million BRUF-BES Construction request, $30.0 million is for the 1-2 GeV Synchrotron Radiation Source. The unique capabilities at this new facility will allow investigators from industry, universities and national laboratories to carry out new studies in many fields, including materials

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Table 15
Basic Research User Facilities (BRUF)
Basic Energy Sciences
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
National Synchrotron Light Source	$ 18.5	$ 19.6	$ 20.3
High Flux Beam Reactor	10.0	10.9	11.5
High Flux Isotope Reactor/ Transuranium Processing Plant	15.8	22.7	23.7
Intense Pulsed Neutron Source	5.0	5.0	5.4
Stanford Synchrotron Radiation Laboratory	9.8	10.3	10.5
Los Alamos Neutron Scattering Center	1.3	3.0	3.1
Combustion Research Facility	3.5	3.5	3.6
6-7 GeV Synchrotron Radiation Source	2.0	10.0	6.2
1-2 GeV Synchrotron Radiation Source	1.5	2.0	2.0
Subtotal	$ 67.4	$ 87.0	$ 86.3
Capital Equipment	6.5	6.7	6.1
Construction	13.3	31.8	48.1
Total BRUF-BES	$ 87.2	$ 125.5	$140.5

Table 16
Basic Research User Facilities
Basic Energy Sciences
Budget Authority
($ in millions)

Project	TEC	Construction Prior Year	FY 1988	FY 1989 Request
6-7 GeV Synchrotron Radiation Source (89-R-601)	$456.0	$ -	$ -	$ 6.0
Accelerator Improvement Projects (89-R-600)	1.6	-	-	1.6
Accelerator Improvement Projects (88-R-600)	2.3	-	2.3	-
3 GeV Spear Injector (88-R-403)	14.0	-	3.0	6.5
1-2 GeV Synchrotron Radiation Source (87-R-406)	98.7	1.5	18.0	30.0
Neutron Scattering Experimental Hall (87-R-403)	17.5	5.0	8.5	4.0
Total Construction		6.5	31.8	48.1

science, atomic and molecular physics, chemistry and biology. The FY 1989 request will permit continued construction and procurement activity leading to completion of the facility in 1992.

The Construction request also includes $6.0 million to initiate construction of the 6-7 GeV Synchrotron Radiation Source. This new facility would not only provide an advanced scientific capability unmatched in the United States, it also would provide much needed additional capacity in the hard x-ray region. As a third generation radiation synchrotron source, it will be used by many scientific disciplines to investigate new materials, surfaces, biological molecules, chemical reactions and geological materials. For these studies, the unprecedented quality of the x-ray radiation, including the intensity, coherence and time structure, will certainly provide the U.S. with important new discoveries. The need for this facility has been agreed upon by several organizations, including the National Academy of Sciences and the Department's Energy Research Advisory Board. This facility has received the highest priority recommendations from these groups.

The BRUF-BES Construction request also includes $6.5 million to continue construction of the 3 GeV SPEAR Injector so that synchrotron radiation experiments at Stanford Synchrotron Radiation Laboratory can proceed independently instead of relying on the availability of the SLAC linear accelerator which also injects into the Positron-Electron Storage Ring and the Stanford Linear Collider; $4.0 million to continue construction of the Neutron

Scattering Experimental Hall at the Los Alamos Neutron Scattering Center; and $1.6 million for accelerator improvement projects at the National Synchrotron Light Source and the High Flux Beam Reactor.

The next program I would like to discuss is Magnetic Fusion Energy.

MAGNETIC FUSION ENERGY

The Magnetic Fusion Energy program is an important part of the Department's efforts to assure an adequate supply of energy for the Nation at a reasonable cost. The Program's goal is to establish the scientific and technological base for fusion energy early in the next century. This schedule would allow a timely assessment of the role of fusion energy in the Nation's energy future.

The Magnetic Fusion Energy program is being conducted according to the Magnetic Fusion Program Plan which was developed in 1985. This Plan took into consideration the constraints on Federal spending, the uncertainty of energy supplies, the excellent technical progress in fusion research, the Program's ability to train creative scientists and engineers, the contribution of fusion to basic research in the fields of plasma physics and atomic physics, and the important role of this Program in technology transfer to industry.

The Program Plan elaborates on the strategy for providing the scientific and technological base for fusion. This two-fold strategy is: 1) to maintain a strong domestic R&D program to adequately cover the necessary range of fusion science and technology issues and 2) to use international collaboration to advance the program in a timely way.

The Fusion Program Plan and the Department's strategy for meeting its goal have undergone several reviews recently. The Department's Energy Research Advisory Board (ERAB) conducted its Congressionally mandated triannual review in the last year. ERAB also looked at fusion as part of its review of the National Research Council report "Physics Through the 1990's." In addition, the Magnetic Fusion Advisory Committee held numerous reviews. All of these groups have endorsed the Department's Plan and strategy for meeting its fusion goals.

A strong domestic program, as called for by the Department's strategy, must include work on each of four key technical issues: the properties of burning plasmas, magnetic confinement systems, fusion materials, and fusion nuclear technology. The FY 1989 budget request provides for a U.S. Magnetic Fusion Energy program focused clearly on these key technical issues and interwoven with other world fusion programs through international collaboration. Let me take a moment to comment on these issues.

The first issue concerns the properties of burning plasmas. An understanding of these properties is required to complete the scientific base for fusion. No experimental facility presently exists anywhere in the world to investigate

this fundamental issue. The United States is in the best technical position to proceed with such an experiment, utilizing the scientific personnel and infrastructure at Princeton Plasma Physics Laboratory (PPPL). As part of joint international planning to avoid duplication of costly facilities, the European Community and Japan have encouraged the United States to undertake a Compact Ignition Tokamak (CIT) to provide critical information to support operation of a next step engineering test reactor, now called the International Thermonuclear Experimental Reactor (ITER). The ITER is essential for completing the technical base of fusion. The U.S. has proposed a joint effort, including the Soviet Union, Japan, and the European Community, to develop a single conceptual design for an ITER by the end of 1990. I will discuss the status of this proposal later in my statement.

Achievement of ignition has always been a major objective of magnetic fusion research. Furthermore, CIT is the only proposed experiment in the world-wide program aimed at addressing the most fundamental scientific issue for fusion, that of understanding burning plasma physics. It is also the natural and cost-effective follow-on to the Tokamak Fusion Test Reactor (TFTR) program. We have now reached the point where experimental progress justifies proceeding with an ignition experiment. The Department believes that CIT is critical to the development of fusion energy and to the U.S. Magnetic Fusion Energy program, and it should be noted that CIT is the only deuterium-tritium experiment proposed for the period between the TFTR in the U.S. and a future engineering reactor.

The schedule for the CIT project, for which the architect-engineering design has been initiated this fiscal year, is influenced by both domestic and international program considerations. A timely U.S. CIT would provide vital information to the world program on how to operate an ITER in the ignited mode and would, thus, save several years of costly exploratory research on the reactor itself. The CIT schedule should allow the facilities and personnel at PPPL, which will complete the TFTR program in 1991, to be productively employed in addressing the important issue of understanding burning plasma physics. The Magnetic Fusion Advisory Committee, the Energy Research Advisory Board, and an international scientific review team have all endorsed the scientific importance of this experiment. The funding proposed in this budget request would allow the U.S. to use the information base available from current experiments both to prepare for its own next step and to support the world fusion program.

The second key technical issue concerns magnetic confinement systems Although significant progress continues to be made in understanding the confinement process, we do not have enough scientific knowledge at this time to design a fusion reactor that is suitable for commercial application. We must, therefore, continue to investigate several confinement systems at modest levels in order to provide this scientific knowledge. Research on this key issue is being closely coordinated internationally to maintain the broadest scientific coverage at minimum cost.

The third key issue concerns materials for fusion systems. Materials research is vital to a successful experimental Magnetic Fusion Energy program, as well as to realizing the ultimate benefits of fusion. Achievement of the Program goal will require the development of new materials to reduce the induced radioactivity and, thereby, enhance the economic and environmental potential of fusion, as well as the development of the facilities required for testing these materials. As part of the Program's international strategy, this issue is being pursued through cooperative agreements. The proposed FY 1989 budget supports the core program needed for U.S. participation in this international effort.

The fourth key issue concerns the nuclear technology of fusion systems. Resolution of this issue requires advances in the basic engineering sciences, as well as application of the results of basic fusion materials research. This issue will be completely resolved only when fuel-producing blankets are integrated into other fusion systems and tested in a nuclear environment, such as ITER. A fusion blanket performs several functions, including converting the energy released by a fusion reaction into useful energy, creating part of the fuel for the reactor, and allowing for recovery of this fuel, as well as shielding critical components from radiation. Our FY 1989 budget request supports the basic technology research that underlies the development of integrated blankets for the long-term and for the ITER conceptual design and validating R&D process in the near-term.

International collaboration has become a major resource for the development of fusion energy. The fusion program has a long history of scientific exchange and cooperation. The Economic Summit process has provided a mechanism for developing an integrated fusion program for the Western allies. Through a series of important international collaborative arrangements, the program has made a great deal of progress toward this goal in the past four years. As noted earlier, the U.S. has proposed a joint effort including the USSR, Japan and the European Community to develop a single conceptual design for the ITER by the end of 1990. After formal discussions among the four parties, the International Atomic Energy Agency issued an invitation to the parties to participate. The U.S., Japan, the European Community, and the USSR all have accepted the invitation. We plan to begin a joint design effort on May 2, 1988. This budget provides for continued U.S. participation in the conceptual design and validating R&D for an international ITER during FY 1989.

The FY 1988 Appropriation Act directed the Department of Energy to submit a five-year budget plan showing how the Compact Ignition Tokamak could be built while completing the Tokamak Fusion Test Reactor program within current budgetary resources. A summary of the plan is included in the FY 1989 budget (Table 17). This plan has been developed recognizing the need for both a balanced base program and preparation for a future program. It focuses the U.S. Magnetic Fusion Energy program strongly on ignition physics issues based on TFTR and CIT. The plan maintains a limited number of recent initiatives aimed at improving toroidal confinement and continues awareness programs in long-range technology. International commitments, including participation in ITER, will all be met under this plan.

Table 17
MAGNETIC FUSION ENERGY
OUTYEAR BUDGETS/(ESCALATED DOLLARS)
(dollars in millions)

	FY 1988	FY 1989	FY 1990	FY 1991	FY 1992	FY 1993
Compact Ignition Tokamak Total	15.5	27.0	46.0	64.0	95.0	101.0
Tokamak Fusion Test Reactor	72.1	74.3	83.0	79.0	45.0	42.0
Total Ignition	(87.6)	(101.3)	(129.0)	(143.0)	(140.0)	(143.0)
Princeton Beta Exp.	11.1	11.4	-	-	12.0	13.0
Alcator C-Mod.	10.1	15.5	16.0	16.0	17.0	17.0
Adv. Toroidal Fac.	18.1	18.0	19.0	19.0	20.0	21.0
DIII-D	29.7	32.5	33.0	35.0	46.0	48.0
Microwave Tokamak Exp.	9.3	10.5	10.0	11.0	-	-
International/Other	7.2	6.6	6.0	7.0	8.0	8.0
Mirrors	3.6	3.0	3.0	-	-	-
Conf. Sytems Total	176.7	198.8	216.0	231.0	243.0	250.0
Base Dev. & Tech.	42.4	42.3	41.0	40.0	41.0	44.0
International Thermonuclear Exp. Reactor**	16.0	16.0	16.0	17.0	18.0	19.0
Dev. & Tech. Total*	58.4	58.3	57.0	57.0	59.0	63.0
Applied Plasma Physics*	78.0	80.6	79.0	80.0	82.0	87.0
General Plant Projects/ General Purpose Equip./ Program Direction	21.9	22.3	23.0	24.0	25.0	26.0
Office of Fusion Energy Total	335.0	360.0	375.0	392.0	409.0	426.0

* Basic R&D in support of the CIT program will be continued, as appropriate.
**Conceptual design report for ITER to be completed in 1990; outyear projections include level of effort, no construction.

In summary, this budget provides for a U.S. fusion program focused clearly on the key technical issues and interwoven tightly with other world fusion programs through international collaboration.

FY 1989 MAGNETIC FUSION ENERGY BUDGET REQUEST

The FY 1989 budget request for Magnetic Fusion Energy is $360.0 million. The request includes $317.5 million for Operating Expenses, $21.6 million for Capital Equipment and $20.9 million for Construction (Table 18). This request will support experimental investigations that are essential to continued research progress and will maintain a balance between our domestic science and technology program and foreign programs with the goal of allowing international collaboration to supplement our domestic research activities.

The request of $360.0 million will allow us to continue work on the four key technical issues discussed earlier in my statement. It will support construction of the Compact Ignition Tokamak device and efforts to develop a common international conceptual design for ITER. To maintain progress at this level of funding, the Program continues to be focused primarily on the investigation of toroidal confinement systems. In addition, we continue to focus the Program on the highest priority tasks, improve the cost effectiveness of facility operations and increase the use of international collaboration in all areas.

Table 18
Magnetic Fusion Energy
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses	$315.4	$298.4	$317.5
Capital Equipment	18.0	19.7	21.6
Construction	8.2	16.9	20.9
Total	$341.6	$335.0	$360.0

The Operating Expenses request of $317.5 million reflects about a 6 percent increase. This increase provides for constant dollar funding for most elements of the program and for increases to meet project funding requirements associated with R&D support and construction of the CIT, preparations for deuterium-tritium operations in TFTR, fabrication of the Alcator-C Mod device, and of the Confinement Physics Research Facility. Work in the technology development area will focus on only the most critical feasibility issues, while providing support for CIT and the Presidential initiative to design and conduct validating R&D for ITER.

Let me now summarize the Operating Expenses request for each of the interrelated functional areas within the Magnetic Fusion Energy program: Confinement Systems, Applied Plasma Physics, Development and Technology, Planning and Projects, and Program Direction (Table 19).

The Confinement Systems subprogram, with an FY 1989 Operating Expenses request of $175.6 million, supports large-scale physics experiments to address key physics issues associated with heating and containing high-temperature plasma. Confinement Systems also develops an information base on reactor-relevant plasma conditions.

In FY 1989, continued studies related to the confinement of high-temperature plasmas will be conducted at the TFTR at Princeton Plasma Physics Laboratory, with preparation for the deuterium-tritium breakeven experiment and studies of plasma confinement at increasing power levels. TFTR has already achieved central plasma ion temperatures of over 300,000,000°C, a value which is more

Table 19
Magnetic Fusion Energy
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Confinement Systems	$184.8	$158.6	$175.6
Applied Plasma Physics	75.2	74.8	78.2
Development and Technology	50.5	55.9	54.1
Planning and Projects	0.9	4.5	5.0
Program Direction	4.0	4.6	4.6
Subtotal	$315.4	$298.4	$317.5
Capital Equipment	18.0	19.7	21.6
Construction			
Compact Ignition Tokamak	0	8.0	12.0
General Plant Projects	8.2	8.9	8.9
Subtotal	$ 8.2	$ 16.9	$ 20.9
Total	$341.6	$335.0	$360.0

than adequate for a commercial thermonuclear reactor; and also has achieved world record values of the fusion confinement parameter which are also adequate for achieving ignition. Breakeven with deuterium-tritium fuel is expected in 1991. The FY 1989 Operating Expenses request also continues the design and R&D effort aimed at building the CIT. Aspects of the CIT design, experimental objectives, and physics studies are specifically directed, wherever possible, to be relevant to ITER.

At the DIII-D device at General Atomics, scientists will continue to explore the ratio of plasma energy to magnetic energy, a ratio characterized as "beta." Higher magnetic fields require larger, more costly magnets. Therefore, experiments are carried out with alternate plasma shapes and operating modes predicted to increase beta. In 1987, a world record beta of 6.2 percent, compared to the previous record of 5 percent, was achieved and sustained without disruptions for several hundred milliseconds. A vigorous experimental program to assess the properties of high-temperature, high-pressure, non-circular plasmas in support of CIT design work is continuing at DIII-D in collaboration with the Japanese.

In addition to the facilities already mentioned, fabrication of the Alcator-C Modification at the Massachusetts Institute of Technology (MIT) also will continue. This facility will confirm various aspects of the CIT design, physics assumptions, and projected operating modes. The Microwave Tokamak Experiment, being fabricated at Lawrence Livermore National Laboratory (LLNL) using the old Alcator-C device from MIT, will be completed in FY 1988 with initial operation expected in FY 1989. This device will provide the unique

capability to test the application of pulsed high power microwaves as a more efficient heating technique in tokamak devices. The FY 1989 request also provides for continued operation of the Advanced Toroidal Facility (ATF) stellarator at Oak Ridge National Laboratory (ORNL) and the Princeton Beta Experiment at PPPL. The Major Device Fabrication for the ATF was completed in December 1987. Plasma operation was initiated in January 1988 with 200 KW of electron cyclotron heating, and neutral beams were injected in February. Research at ATF will contribute to the advancement of toroidal physics and to the international collaborative effort on stellarators.

International collaboration will be relied on to carry out research on a number of related plasma issues in foreign facilities including TEXTOR and ASDEX in West Germany, Tore Supra in France, the Joint European Torus (JET) in Europe and JT-60 in Japan. In just one example of the ongoing collaboration at these facilities, ORNL and PPPL have fabricated and installed a 3-barrel, high speed pellet injector on JET as a part of the U.S.-European Community cooperation. Last December, the first in a series of pellet injection experiments resulted in the highest density in a JET plasma with ion cyclotron heating. These experiments, which are essential to achieving the required high fusion confinement parameter, are continuing.

The Applied Plasma Physics subprogram, with an FY 1989 request of $78.2 million, develops plasma physics theories and models to predict and explain the experimental and theoretical investigations of basic physics problems related to confinement concepts. This subprogram also conducts research on

magnetic confinement configurations with potentially improved reactor features, provides computational support to the program, and develops diagnostic instruments for use by the Confinement Systems subprogram.

Significant progress has been made in understanding physical processes that reduce confinement of particles within tokamaks. Several advanced diagnostic techniques were applied by a collaboration of university groups at the TEXT tokamak to obtain the new measurements. Through theoretical efforts, these insights will impact our understanding of the critical, related issue of energy confinement in tokamaks.

Exploratory experiments on a small toroidal device recently found highly efficient production of the plasma current, which is required to form the tokamak configuration, through application of an electron "beam" at the plasma edge. Improved current drive efficiency is a critical reactor feature for tokamaks. These initial experiments will be advanced to a moderate sized tokamak during FY 1989.

The spheromak, a compact toroidal plasma with no hardware on the inside of the torus, is one of the alternate confinement concepts being investigated under this subprogram. The S-1 experiment at Princeton has completed a 4-year research program that demonstrated plasma compression heating, one of several methods of heating the plasma, and confirmed the pioneering theory of "energy minimum" confinement. This theory determines generally how the magnetic fields and stored energy evolve in all toroidal plasmas.

Other alternative confinement concepts that will continue to be investigated in FY 1989 are the Reversed Field Pinch (RFP) and Field Reversed Configuration (FRC). Both devices make efficient use of magnetic fields. In addition, RFP devices require no auxiliary heating, while FRCs have simple magnetic fields and offer natural systems for impurity control. The FY 1989 request for the Applied Plasma Physics subprogram reflects a slight increase primarily to continue fabrication of the Confinement Physics Research Facility (CPRF) at Los Alamos National Laboratory. Support for the theory, experimental research and computer network elements of this subprogram will continue at a flat level with some decreases in scope of work to accommodate the increased cost of operations.

The Development and Technology subprogram, for which $54.1 million is requested in FY 1989, develops components and systems for use in near- and mid-term fusion experiments, as well as long-term generic materials and fusion nuclear technology. This subprogram also performs conceptual design studies for future fusion devices. These design studies identify component and system development needs that must be met to prepare for the eventual application of fusion.

In FY 1987, the International Energy Agency Large Coil Task testing program at ORNL was completed to the satisfaction of all participants, the U.S., Japan, the European Community, and Switzerland. Based on the results of this program, we are now confident that we can design and build the required large superconducting magnets for fusion.

Organization of the U.S. ITER activities has been completed, including Technical Study Tasks and R&D to validate design concepts. In FY 1989, design and R&D work will continue in support of ITER. These efforts will include international collaboration on development of high field, steady state magnets; research on improved techniques for heating, current drive and plasma control; development of higher performance pellet injectors; research on tritium recycling technology and safety and environmental issues; and materials development in support of the ITER design.

The Planning and Projects subprogram primarily provides support for construction of major new facilities and fabrication of auxiliary components for these facilities. The FY 1989 Operating Expenses request for $5.0 million is for the support of special program needs and for compliance with legislative requirements associated with the Small Business Innovation Research (SBIR) program.

The Operating Expenses request of $4.6 million for Program Direction provides for personnel and other costs associated with continuation of 62 full-time equivalent staff.

The FY 1989 Capital Equipment request of $21.6 million will provide for essential hardware necessary to support the Program, such as power supplies, diagnostic and computer equipment and components for experimental facilities. The increase in the Capital Equipment request is primarily in the Toroidal Confinement Systems area and the Development and Technology area.

The FY 1989 Construction request of $20.9 million provides $12.0 million for the Compact Ignition Tokamak that I discussed earlier. A national design team has been working for several years to develop a low-cost concept that would provide a compact, high field, copper coil tokamak facility to generate critical burning plasma data to allow for successful operation of an integrated ITER. The project, with a TEC of $426 million, began in FY 1988 and is scheduled to be completed in FY 1996. The funds requested for FY 1989 will provide for design, long-lead-time procurement, and initial construction of the CIT device components and systems. The remainder of the Construction request, $8.9 million, is for General Plant Projects. These projects at LLNL, PPPL and ORNL provide for the continuing minor alterations and modifications necessary to protect the government's investment in its facilities.

Now let me turn to the Biological and Environmental Research program.

BIOLOGICAL AND ENVIRONMENTAL RESEARCH

The Biological and Environmental Research (BER) program supports research aimed at accomplishing two important goals. The first goal is to develop the basic knowledge necessary to identify, understand and anticipate the long-term health and environmental consequences of energy use and development. The second is to use the Department's unique scientific and technological capabilities to solve major scientific problems in medicine and biology.

Over the years, the BER program has greatly improved our ability to understand and predict the long-term health and environmental effects of energy technologies. Researchers supported by this Program have, among their many contributions: (1) produced most of the health effects data needed to establish the human radiation exposure standards in world-wide use; (2) developed most of the radiation detection systems and measurement techniques used to protect nuclear industry workers and the general public; (3) established the concept of studying the environment as an interactive, integrated medium for the cycling and transport of pollutants; and (4) provided training and research experience for most of the Nation's radiation biologists, radiological physicists and radioecologists.

The resources used by the Department to develop new scientific knowledge and technologies, such as the examples I have just mentioned, have also made major contributions to the study, diagnosis and treatment of human diseases, and to the resolution of major problems in the biological and environmental sciences. BER's contributions in nuclear medicine are a clear example. This research, based on the Department's capabilities in high energy physics, radiation chemistry and isotope separation, has produced the technology for the millions of nuclear medicine diagnostic and therapeutic procedures performed annually. In addition to the obvious human benefits of nuclear medicine procedures, these developments have also helped create a substantial industry that supplies radioisotopes and nuclear medical equipment.

Although research results cannot be predicted on an annual basis, the Program continues to produce impressive results in the form of new scientific information as well as instrumentation and biomedical technologies that will allow researchers to continue to expand our knowledge base. Let me summarize just a few of our recent accomplishments.

At the University of Chicago, researchers have developed a computational approach that can correlate medical images obtained by three techniques--Computer Assisted Tomography (CAT), Positron Emission Tomography (PET), and Magnetic Resonance Imaging (MRI)--and display the combined structural and functional information simultaneously. Because these images are acquired at different times, in different formats, and from different perspectives, they are difficult to compare visually. This technique has already proven useful in evaluating radiation treatment of brain tumors, and is expected to have other diagnostic applications.

Researchers at Lawrence Livermore Laboratory have developed a simple way to assess the cumulative somatic cell genetic damage caused by exposure to ionizing radiation. (Somatic cell mutations, unlike mutations induced in reproductive cells, cannot be transmitted to one's offspring.) By identifying red blood cells that are lacking a specific protein due to mutations caused by exposure to radiation, and counting the mutant cells in small blood samples, scientists are able to estimate the accumulated mutations in the bone marrow of the exposed individuals. Studies of groups of people exposed to ionizing radiation, such as the Hiroshima survivors and the firefighters at Chernobyl, show increased incidences of non-heritable mutations in proportion to the dose

of radiation they received. To verify the expectation that these genetic changes will persist through the life of the individual, long-term studies on the exposed individuals are planned in conjunction with the International Atomic Energy Agency.

The importance of DNA repair mechanisms in protecting cells from radiation damage has been dramatically demonstrated by Lawrence Berkeley Laboratory (LBL) scientists. Analysis of research data at LBL revealed that removal of the genes responsible for the DNA repair process from yeast cells leaves those cells 1,000 times more vulnerable to ultraviolet radiation from sun light. Because DNA repair processes in yeast cells are similar to those in human cells, this finding is important for the understanding of skin cancer and other diseases triggered by ultraviolet light, and by other environmental agents such as ionizing radiation and certain chemicals.

New information about the pivotal role played by atmospheric oxidants, such as hydrogen peroxide, in the production of acids in clouds has made an important contribution to the National Acid Precipitation Assessment Program (NAPAP). The work was conducted by researchers at Brookhaven National Laboratory. Hydrogen peroxide promotes a rapid transformation of sulfur dioxide to sulfuric acid. During special weather conditions, primarily during the winter, hydrogen peroxide concentration can be so low that it cannot maintain the production of sulfuric acid over large areas despite the abundance of sulfur dioxide. This has important implications to contemplated emission control strategies as during these low oxidant periods reducing sulfur dioxide emissions will have negligible impacts on sulfuric acid deposition.

With its focus on the earth rather than the atmosphere, exploratory research begun in 1986 on the microbial ecology of the earth's crust has generated international attention in the scientific community and in the private sector. The discovery by scientists at Pacific Northwest Laboratory of diverse microbial communities at depths of nearly 900 feet opened the possibilities for in situ cleanup of organic chemical contamination in deep burial sites. During the past year, findings of bacteria with antibiotic resistance raise the possibility of finding other microorganisms producing novel, medically useful compounds.

The FY 1989 budget request for the BER program is $249.0 million. Within this request, $236.5 million is for Operating Expenses, $9.0 million is for Capital Equipment, and $3.5 million is for Construction (Table 20). Funding by research area is shown in Table 21. The base BER program reflects an increase of approximately 15 percent, excluding the FY 1988 Congressionally initiated university construction projects.

The broad scope of BER's mission can be seen in the several research areas for which funding is requested in FY 1989. Research related to the long-term effects of energy development and use on human health and the environment will be carried out under four research areas: Source and Dose Determination, Environmental Processes and Effects, Health Effects and General Life Sciences. The FY 1989 budget request in these areas will support research and the development of instrumentation to: (1) identify, measure, and characterize energy-related contaminants such as radiation and toxic chemicals; (2) enable

Table 20
Energy Supply R&D
Biological and Environmental Research
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses	$176.6	$204.6	$236.5
Capital Equipment	8.5	8.5	9.0
Construction	6.0	56.2	3.5
Total	$191.1	$269.3	$249.0

Table 21
Energy Supply R&D
Biological and Environmental Research
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Late Effects on Health & Environment of Energy Development and Use			
Source and Dose Determination	$ 13.6	$ 16.0	$ 17.7
Environmental Processes & Effects	31.8	32.4	41.0
Health Effects	59.5	64.2	70.7
General Life Sciences	33.3	39.8	51.0
Nuclear Medicine	21.8	34.2	34.5
Carbon Dioxide Research	13.0	14.0	17.6
Facility Operations	14.5	64.7	12.5
Program Direction	3.6	4.0	4.0
Total	$191.1	$269.3	$249.0

us to predict how these materials move through and affect the environment; and (3) understand their potential effects on human health. The FY 1989 request also will support research aimed at adding to our understanding of variations in human susceptibility and resistance to environmentally associated diseases and our ability to predict and modify the possible genetic burden that current policy places on future generations as well as our own.

I would like to highlight some key aspects of the FY 1989 request in the environmental and health research areas I have just mentioned. In the environmental area, the funds requested in FY 1989 will be used to address a variety of issues, including: acid rain, indoor radon, carbon dioxide, global climate change, the resiliency of ecosystems and marine coastal areas to the effects of energy activities, management of radiation and chemical wastes, and atmospheric modeling at different terrains representative of different energy development sites. Among other FY 1989 activities, there will be a substantially increased effort to define the role of microorganisms living in the earth's subsurface in degrading, or breaking down, chemical contaminants so that their harmful effects are reduced or eliminated. This research will have long-term implications for decontamination of deep aquifers.

An ongoing effort to develop critical new information that ultimately can be applied to cost-effective decontamination of DOE wastes and for remedial action will be expanded in FY 1989. The new information gained from this subsurface research will improve our understanding of factors that influence the movement of mixed wastes through soil, thus contributing to safer and more effective operation of shallow, low-level burial sites. This effort will also

include the development of accurate, non-invasive and inexpensive techniques and instruments that will eliminate the need for costly well drilling to assess migration of subsurface wastes. Research to determine the movement of contaminants in the subsurface and factors that limit their movement will be emphasized also.

The radon research program, significantly expanded in 1988 in response to increased national concern, will continue in FY 1989. This program, in accordance with a DOE/EPA Memorandum of Understanding, will emphasize the basic scientific research needed to establish risk from exposure to indoor radon. Research will include identification of critical cells in the lung, improved measurement technology, modeling of radon in soils, and interactions between radon and smoking in lung cancer development.

Another important aspect of the FY 1989 request is the Life Sciences research aimed at developing a better understanding of DNA damage and repair mechanisms. An enhanced effort will be applied toward improving the tools and methodology needed for deciphering the human genome, the complete set of genetic instructions present in human cells.

The human genome effort, formally initiated by BER in FY 1987, is being increasingly recognized by the scientific community as modern biology's most significant challenge. Greater understanding is expected to lead to major advances in all aspects of biology and medicine, including the diagnosis, prevention and treatment of the more than 3000 genetically caused diseases. This activity is also expected to provide a major impetus to the Nation's

biotechnology industry. The new tools and methodologies to be developed under this initiative will greatly increase the pace and lower the cost of mapping and sequencing the human genome. The proposed level of effort of $18.0 million will help establish the phased, multidisciplinary collaboration necessary among academia, the private sector and the Department's laboratories.

In addition to the four health and environmental research areas I mentioned earlier, the BER program also supports research in Nuclear Medicine and Carbon Dioxide Research. The Nuclear Medicine activity, with an FY 1989 Operating Expenses request of $34.5 million, continues to make it possible to monitor body functions, isolate the causes and extent of diseases and treat these diseases with new and more effective methods. The development and use of new radiopharmaceuticals in metabolic studies, chiefly of the heart and brain, is one major aspect of this activity. Radiopharmaceuticals are radioactive compounds that concentrate in certain body organs and can be followed with sensitive radiation detectors to make the body's internal organs visible to medical diagnosticians. This allows diagnoses to be made without invasive procedures such as biopsy. In FY 1989 this research activity will include an increased effort to explore boron neutron capture therapy as a promising radiation treatment.

Research in Nuclear Medicine also includes clinical feasibility studies which involve in vivo testing of new radiopharmaceuticals in animals and, subsequently, in selected patients, as well as efforts to develop and improve instrumentation for the diagnosis and treatment of disease through nuclear

medicine. In addition to research in FY 1989 aimed at improving the resolution of positron emission tomography and other imaging techniques, research on a new portable device for rapid, bedside evaluation of pulmonary edema will be completed. This device will allow water buildup in the lungs to be estimated more accurately than is currently possible. Emphasis will also be placed upon developing monoclonal antibodies for diagnosis and therapy.

The FY 1989 Operating Expenses request for the Carbon Dioxide Research activity is $17.6 million. The goal of this research continues to be to develop sound, quantitative information about atmospheric CO_2 change and its effect on climate, earth resources and energy technologies. We must understand the cause and effect relationships between CO_2 concentrations and global, regional, and seasonal warming before energy policy recommendations can be made.

Carbon dioxide and the greenhouse effect linked to global climate change has become a major national and international science and energy policy issue. The Department's State-of-the-Art Reports, which summarize what we know, show substantial progress but also identify areas where further research is needed to reduce uncertainties. These research needs include a better understanding of how climate is changing and whether those changes are consistent with projections. Substantial improvement also is needed in the way oceans and clouds are portrayed in climate models. A third problem identified by the State-of-the-Art Reports is the need to develop data and evaluate the primary effects of both CO_2 and climate change on biological systems as a whole.

The FY 1989 budget request will support research aimed at improving our knowledge base in each of the above areas. The development of regional resource and analysis techniques will be emphasized as will the efforts aimed at understanding how clouds and oceans affect global exchange and storage of CO_2 and heat. The budget request includes $1.0 million for policy analysis research. This research will investigate the potential reduction in CO_2 emissions that could be achieved through strategic energy planning. It will also identify which geographic regions may be in jeopardy if significant climate changes occur.

The request also includes $2.0 million for research related to the comparison of various predictive climate models and improved data and analysis of clouds and the oceans as they affect and are affected by climate change. The objectives of this research are to: (1) understand the causes and reconcile the disparities among model simulations; (2) provide a comprehensive diagnosis as to needed improvements in the climate models; and (3) identify the uncertainties and limits to the accuracy of seasonal and regional predictions as a means of providing useful data. Results of the leading U.S. and international climate models will be analyzed in cooperation with the research groups developing and applying the models. The international groups include scientists from England, China, Canada, Germany, the Soviet Union and Japan as well as the U.S.

The FY 1989 BER budget request for Facility Operations is $12.5 million. Of this total, $9.0 million is for Capital Equipment and $3.5 million is for General Plant Projects. The Capital Equipment request supports replacement of

deteriorated and obsolete equipment at the laboratories as well as acquisition of new, state-of-the-art instruments. General plant projects are necessary to keep the general plant of the laboratories (e.g., buildings, roads and utilities) in good operating condition.

The FY 1989 request for Program Direction is $ 4.0 million. These funds are required to pay for the personnel costs of 55 full-time equivalents to direct, manage and support BER's multidisciplinary research efforts.

At this point, I would like to discuss the several ER programs that are related to my statutory responsibilities to advise the Secretary on DOE's R&D programs, on the non-weapons multiprogram laboratories and on the education and training of scientists and engineers.

ENERGY RESEARCH ANALYSIS

The Energy Research Analysis (ERA) program performs technical assessments and research analyses that cut across all aspects of the Department's civilian R&D programs. These assessments and analyses provide technical support to the Director in carrying out legislatively assigned R&D oversight responsibilities. The ERA program focuses in depth on particular project or program areas and offers an independent view of how well research is conducted and where the most promising opportunities for future research lie. The program also plays a key role in coordinating the Department's involvement in acid rain research. Specifically, the program provides DOE'S share of the financial support for management of the National Acid Precipitation Assessment

Program (NAPAP). In addition to providing funds, this program represents DOE on technical task groups and reviews documents produced by NAPAP and non-Federal groups for their scientific quality and objectivity.

The FY 1989 budget request of $3.8 million will provide continued funding for assessments of ongoing research on membrane separations technology, laser instrumentation and analytical chemistry of air pollution. In addition, analyses critical to the successful completion of the final National Acid Precipitation Assessment Program in 1990 will be conducted.

UNIVERSITY RESEARCH SUPPORT

The Department encourages universities and colleges to contribute to the national energy R&D effort, for these universities produce much of the new scientific knowledge and technical talent that ultimately enters the marketplace and contributes to the Nation's continued economic and social progress. University scientists and students conduct research sponsored by DOE in all areas of fundamental science and energy R&D. This support includes both direct awards for on-campus research and collaborative efforts involving DOE laboratory scientists and facilities. The Department also supports the involvement of faculty and students in two specific research and science education-related programs, the University Research Support (URS) Program and the related University Research Instrumentation Program, which I will discuss next.

The major objectives of the URS program are to: (1) strengthen university capabilities to perform long-range R&D; (2) enhance the quality and increase the numbers of young people pursuing energy-related R&D careers; (3) use the unique resources of the Department's laboratories both for university faculty and student research and training; and (4) enhance technology transfer to the private sector, thus assisting in the national effort to revitalize the Nation's economic position in the world marketplace.

The URS program, with an FY 1989 budget request of $15.8 million, consists of four activities: Laboratory Cooperative Science Centers, R&D Laboratory Technology Transfer, University Reactor Fuel Assistance, and Energy Manpower Development (Table 22).

The largest of the URS programs, with a request of $9.5 million, is the Laboratory Cooperative Science Centers. The essential roles played by the Department's laboratories in assisting university research and manpower development have been underscored in recent Energy Research Advisory Board and White House Science Council reports on the health of U.S. colleges, universities and industry. Consistent with these analyses and the President's goal of improving our national science and technology research and education base, an initiative was begun in FY 1988 to broaden and strengthen the Laboratory Cooperative Science Centers. This Program provides support for over 2100 summer research/instructional appointments at the DOE laboratories for university faculty and students. In addition, a new semester-length research program was initiated at five DOE multiprogram laboratories. Three hundred undergraduate science and engineering students were selected in a

Table 22
Energy Supply R&D
University Research Support
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Laboratory Cooperative Science Centers	$ 9.9	$13.8	$ 9.5
R&D Laboratory Technology Transfer	0.8	1.0	1.4
University Reactor Fuel Assistance	3.8	1.9	2.1
Energy Manpower Development			
Energy Manpower Assessment	0.5	0.5	0.6
Energy Education and Training	0.6	1.2	2.2
Total URS	$15.6	$18.4	$15.8

national competition. In FY 1989, support will be expanded to provide for 450 undergraduate science and engineering students and 36 faculty/student research teams to participate in the program at six labs. This expanded effort provides a year round focus for students and faculty research and education efforts.

In FY 1989 the R&D Laboratory Technology Transfer activity will provide joint support to 30 industry scientists and engineers to work on research assignments at national laboratories side-by-side with DOE's scientists, with the objective of transferring federally supported expertise to the private sector for commercial application. These assignments are cost shared with industry and involve both large and small companies. Support will also be provided for pilot laboratory initiatives to improve spin-off of federally funded R&D to U.S. industry.

The University Reactor Fuel Assistance activity provides support for refueling and related activities for university nuclear research and training reactors. The FY 1989 budget request will provide for refueling four reactors and will fund 23 reactor sharing research awards involving scientists from more than 50 universities. The request will also continue support for low-enriched uranium reactor fuel conversion as required by the Nuclear Regulatory Commission. This activity will be approximately at its midpoint by FY 1989 with 12 university reactors having received support for their reactor safety studies and four having received Commission approval for fuel conversion and startup.

The goal of the Energy Manpower Development activity is to increase the number of young people pursuing energy-related scientific and technical careers. The FY 1989 budget request will expand support for precollege science student and teacher programs sponsored by the laboratories. Six high school science student honors programs will be supported in FY 1989 at the Lawrence Livermore, Brookhaven, Lawrence Berkeley, and Oak Ridge National Laboratories, Fermi National Accelerator Laboratory, and the Pacific Northwest Laboratory. Over 2100 minority and female junior high school students will be supported through university grants under the Pre-Freshman Engineering program.

UNIVERSITY RESEARCH INSTRUMENTATION

The goal of this annual, competitive program is to strengthen the ability of universities to conduct long-range research on selected energy problems and processes by helping them to acquire the required state-of-the-art instrumentation. Another objective is to provide graduate students with hands-on experience in the use of sophisticated research equipment.

The University Research Instrumentation (URI) program, with a budget request of $5.0 million for FY 1989, complements the Department's energy research and technology programs. Generally, research grants and contracts provide support only for research operating costs, with funding for new equipment limited to instruments costing less than $30,000. The URI program assists universities in purchasing instruments costing over $100,000 that will be used by a number of researchers, including students.

Each year, the Department's program managers help select the high priority energy topic areas that are to be emphasized in the URI program announcement. The annual awards are based on merit, the accomplishments of current DOE-supported research projects, and the degree to which the new equipment will enable researchers to substantially advance the understanding of energy-related phenomena. Since FY 1986, the URI program has emphasized support for innovative instrumentation, that is, equipment not generally available at U.S. universities. The FY 1989 request will continue this emphasis and will support about 18-20 instrumentation awards.

MULTIPROGRAM ENERGY LABORATORIES-FACILITIES SUPPORT

The Multiprogram Energy Laboratories-Facilities Support (MEL-FS) program aims to reduce deficiencies and to maintain safe, cost-effective and productive operations at the five national laboratories overseen by the Office of Energy Research. This is accomplished by supporting activities that counter problems of aging and obsolescence of facilities and of environmental compliance.

The FY 1989 budget request for MEL-FS is $65.9 million. Of that amount, $41.4 million is for the General Purpose Facilities Construction subprogram and $24.5 million is for the ORNL Environmental Compliance subprogram (Table 23).

The General Purpose Facilities Construction subprogram provides for rehabilitation and upgrading of deficient buildings, utilities, roads and other necessary facilities at the laboratories supported by the Office of Energy Research. The highest priority projects are those that address urgent

Table 23
Energy Supply R&D
Multiprogram Energy Laboratories - Facilities Support
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
General Purpose Facilities Construction			
Ongoing Projects	$ 23.7	$ 22.7	$ 29.8
New Projects	7.7	5.8	11.6
Total General Purpose Facilities	$ 31.4	$ 28.5	$41.4
Environmental Compliance-ORNL			
Operating Expenses	14.3	12.9	8.8
Capital Equipment	1.0	0.4	0.3
Construction	10.0	11.8	15.4
Total Environmental Compliance	$ 25.3	$ 25.1	$ 24.5
Total Multiprogram Energy Laboratories-Facilities Support	$ 56.7	$ 53.6	$ 65.9

environmental, health and safety deficiencies and those that can hamper or interrupt operations. The Construction request for this subprogram will continue 19 ongoing projects and initiate 14 new projects. The new projects include 2 environmental related projects, 1 road safety project, 2 fire protection projects, 3 utility projects and 6 building rehabilitation or replacement projects.

The ORNL Environmental Compliance subprogram originated in 1985 to address non-defense environmental deficiencies at Oak Ridge National Laboratory. This program will bring all operational systems up to current and expected environmental standards and will perform remedial actions necessary to clean up environmentally contaminated areas that are related to past ER activities.

Of the total $24.5 million Environmental Compliance request, $8.8 million is for Operating Expenses. This request will provide for assessments of the condition of operating systems and development of plans for needed upgrades. The Capital Equipment request of $0.3 million will provide equipment needed to support cleanup activities. The $15.4 million Construction request will provide funds to continue two high priority construction projects related to the liquid waste systems at ORNL.

ADVISORY AND OVERSIGHT PROGRAM DIRECTION AND POLICY AND MANAGEMENT

The FY 1989 budget request for Advisory and Oversight Program Direction is $3.3 million. The FY 1989 request will provide the staffing resources required by the Director of Energy Research to carry out his responsibilities

under legislation (P.L. 95-91) and those assigned or delegated by the Secretary in areas beyond the scope of other ongoing Energy Research programs. These funds are required to provide for the salaries, benefits, travel and other expenses associated with 44 full-time equivalents.

The FY 1989 budget request for Policy and Management is $0.7 million. These funds are required to provide for the salaries and related expenses associated with 8 full-time equivalents in the Office of the Director of Energy Research.

That concludes my statement. I would be happy to answer questions.

Senator FORD. Thank you very much, Dr. Decker. If the full Administration's request for the SSC is approved, and this is the core of the problem when we look at the amount of money that is being suggested, and reductions in the other areas, so that is the reason I am going to lean very heavily on that point this morning.

This request, if approved, will almost certainly result, in my opinion, in an energy and water development appropriations bill for fiscal year 1989 that is over budget, if we accept that. Will the President support that?

Dr. DECKER. Will he support——

Senator FORD. Over budget?

Dr. DECKER. Over budget? I believe, as the Administration's budget request was submitted to the Congress, that it was within the agreement of the budget summit.

And I know that that included a rather large increase for science as a whole, and I expect that that came as a result of cuts in other areas.

Senator FORD. Well, of course, selling NPR, cancelling that, plus the Appalachian Regional Commission, and doing a lot of other things, Doctor, fits within the budget. And you know as well as I do that Congress is not going to allow that.

And so therefore we get back to—do we keep this full amount, or do we fund some of the others? If we keep the full amount on SSC and fund the others, then that puts us over the budget.

And do you think under those circumstances the President will sign it?

Dr. DECKER. That is hard for me to predict.

Senator FORD. So as a diplomat, your answer is, yes he will veto it?

Now, the appropriations and budgeting process force the SSC to compete with the alternative science projects. In the absence of any new money in the system, Congress has to set priorities. We have to pick and choose.

What kind of picking and choosing, if I can use that term, was done by the Administration when the decision was made to request the SSC funding?

Dr. DECKER. I think that the Administration has stated its position that it does not believe that SSC should come at the expense of other areas of science.

And I believe that what the Administration has done is to go forward with other projects in science that it feels are worthwhile, things the country should move forward with, and has essentially added the SSC to that request.

Senator FORD. Is it, in the words of Secretary Herrington, that there is no reduction for worthy projects, and then the worthy project then becomes—it is in the eye of the beholder then, is it not?

Dr. DECKER. Yes.

Senator FORD. There has to be some judgment made by the Department in order to stay within the budget that some projects were kept and some were dropped or delayed.

And when the Administration chose the funding for the SSC, what is "no worthy project was hurt by the SSC"? What does the Secretary mean by that?

Dr. DECKER. Well, the way the budget process worked in the Department, the budget targets that we had for our R&D programs were established, I believe, last January before the SSC decision was made.

The Office of Management and Budget added the SSC money to the Department's overall target. And it did not affect the targets that had been previously established for our other R&D programs.

They have come in pretty much, I believe, at the target that was established much earlier by the Office of Management and Budget.

Senator FORD. Well, let us take a project that is nearby and near and dear to, I guess, Senator Warner, the CEBAF, which is in his commonwealth, was supposed to have $60 million. That was reduced to $44 million.

Do you think he is going to look kindly on that?

Dr. DECKER. Probably not.

Senator FORD. I think that is putting it mildly.

Dr. DECKER. That may be. But the SSC funding did not influence what was available for that particular project.

The decision was made on the basis of priorities within the existing targets for the Nuclear Physics and other programs in Energy Research, and SSC was kept completely on the side, as a separate issue.

So if the decision had not been made to move forward with SSC, I think the situation with that project would have been the same.

Senator FORD. So we are going to tell Senator Warner that regardless of SSC you were going to cut that project from $60 million to $44 million?

Dr. DECKER. It would have turned out that way, in my view.

Senator FORD. Now, what did OMB tell you, or where did OMB tell you to find the money? Or did they just tell you this is where we are going to cut and this will make the money, make up the difference?

Dr. DECKER. For the SSC project?

Senator FORD. Yes.

Dr. DECKER. Well, they never told us specifically where they were making cuts in other areas of the Federal budget, if that is, in fact, what they did to provide money for SSC.

I think that the one thing that we were assured of was that it was not coming from other areas of science.

Senator FORD. Something about that statement bothers me just a little bit, Doctor, when we see all of these reductions in order to get the money for the SSC. They kind of balance off. And it seems strange that you would make those judgments prior to SSC coming into the financial picture.

Dr. DECKER. We normally get our budget targets from OMB well in advance of when we put our budget request together. I do not know what more to say about that.

Senator FORD. Well, Doctor, we had a budget agreement last year, and we are supposed to have smooth sailing this year. The next President is the one that is going to get his buns knocked off. [Laughter.]

But this money has to come from the science community, the reductions, in order to make it up. I think that is the agreement.

And then when OMB gave you this money, as I understood you, or they put it into your budget, then you had to come out with a cap. So you had *x* number of dollars in the scientific community in order to meet this new money, then something had to be curtailed or cancelled.

Dr. DECKER. That is not my understanding of the way it worked. There is a rather substantial increase in the total science budget that has been proposed by the Administration, which includes not only increased funding for SSC but includes a substantial increase for the National Science Foundation, NASA, and other science programs as well.

Senator FORD. Well, let us just look at it this way. It seems unlikely to this Senator that you are going to get the full funding that you have requested.

Suppose you get $25 million to $50 million to continue SSC research and development, and we let the next President take a look at the scientific priorities and new sources of funds, what would the problem be with that?

Dr. DECKER. I think there are a couple of problems with it. First of all, it obviously slows the project substantially and destroys any momentum that I think we have built up.

But another important aspect is that we are trying very hard to get participation by other countries in this project. Unless we get some sort of a commitment to move forward with construction at some level, I think we will have a very difficult time convincing other countries that, in fact, we have a viable project and that they should join us in helping to fund it.

Senator FORD. Well, we have some indication that this project might survive with as little as $150 million. Do you have any comment as relates to that?

Dr. DECKER. Well, certainly the Administration has proposed a budget that it feels would keep this project on a good schedule. $150 million would obviously delay it some.

I think that if, within that $150 million, there is a good chunk of construction funding, then we could at least keep momentum in the project, and I think we would have a reasonable basis on which to go back to some of the foreign countries and try to convince them that, in fact, we have a good project that is moving forward.

Senator FORD. Well, Doctor, SSC was at $25 million last year, and $150 million this year is a pretty good increase.

Dr. DECKER. Yes, sir.

Senator FORD. And I just would hope that we could find some way to keep some of the other projects moving forward. And I am getting the indication from you that $150 million might not be too bad. Not too good, but not too bad.

Dr. DECKER. Well, it is not too good. But we also recognize the budget constraints that we all face. So, we will try to work with you.

Senator FORD. Well, I am pleased that everybody understands the budget constraints.

In your statement you indicated that other countries 'contribution would be in-kind. Is there any prospect of anybody putting out a little cash? Or is it all our cash?

Dr. DECKER. No, I think there is some prospect of that. But I think the larger contributions are likely to be in-kind contributions.

And that has typically been the easiest way to facilitate international participation in projects. It is easier for countries to construct the piece of equipment and ship it to another country for use in a scientific project, rather than to ship cash.

Senator FORD. It would be so much easier to just put it in small bundles. [Laughter.]

The in-kind means personal service of scientists and that sort of thing, in lieu of cash?

Dr. DECKER. Yes.

Senator FORD. Now, would those people come to this country and work here? Or would they work there and then send their data here?

Dr. DECKER. No, I think that we would have a lot of scientists being sent here by other countries. So the in-kind contributions can be in the form of equipment or it could be in terms, as you are suggesting, of a scientist coming here and helping us on the project.

Senator FORD. Say we work it out and things begin to move, and we get the commitment that is sufficient for other countries to be willing to make a commitment, under that scenario, do you think that will reduce the costs of the overall project? Or if you want, year by year in the overall project?

Dr. DECKER. The honest answer is that I do not know. We have had a number of informal discussions with quite a few countries over the last several years.

It has only been the last few months that we have opened formal discussions. And the discussions have really just begun. If I were to estimate, based on some of the informal discussions that we have had, it is a number that is maybe 25 to 40 percent.

Senator FORD. Now, your estimate, the total cost of the project if $5,320,000,000. Is that correct?

Dr. DECKER. That is correct.

Senator FORD. And you expect a non-Federal estimate of $1.8 billion?

Dr. DECKER. Yes, that is the total non-Federal contribution that was estimated.

Senator FORD. And so you are looking at us to go from $25 million in 1988, $363 million in 1989, $675 million in 1990, $475 million, $450 million, $490 million, $543 million, $294 million, and $205 million through year 1996. That is from your budget program.

Dr. DECKER. That sounds right, yes.

Sources of Funding
for the SSC

(estimated in Department of
Energy FY 1989 Budget Request)

($ in millions)

Year	Federal	Non-Federal (estimated)	Total
1988	25	0	25
1989	363	0	363
1990	675	0	675
1991	475	299	774
1992	450	359	809
1993	490	389	879
1994	543	403	946
1995	294	300	594
1996	205	50	255
TOTAL	3520	1800	5320

Senator FORD. Let me ask you another question or two about the budget you propose for DOE science programs.

Do you think you make the best use of your existing facilities?

Dr. DECKER. We try very hard to do so. We always are in a position of making difficult tradeoffs, within constrained budgets, between full utilization of our existing facilities and moving forward with the new facilities that the country clearly needs in the future.

And so I think that in the High Energy Physics Program, in fact, I know that, in the High Energy Physics budget request for fiscal year 1989, we have planned for full utilization of Tevatron and the Stanford Linear Collider.

Senator FORD. Well, is this statement true, then, Doctor, that for a very modest additional amount existing facilities could produce a lot more work? Just a few million dollars?

Dr. DECKER. That depends a little bit on which program you are talking about. I think as far as high——

Senator FORD. Material sciences, let us talk about that.

Dr. DECKER. In the Material Sciences Program, we do have a particularly tight budget in fiscal year 1989. That in part came about because we had some last-minute expenses that we clearly had to meet to bring the High Flux Isotope Reactor at Oak Ridge up to appropriate safety standards.

That is one of our old reactor facilities, and it became clear to me that we had to put an additional $6 million or $7 million into that project at the last minute. That did take away from our operating budgets for some of the other facilities, and those other facilities are tight as a result of it.

Senator FORD. Is that where you studied the superconductivity?

Dr. DECKER. Yes, some of those facilities are used to study the new superconductive materials.

Senator FORD. Would it be important to find a few million dollars to get the most from them?

Dr. DECKER. I would like to be able to do that, yes. But as I say, my overall budget constraint was such that it was hard to do.

Senator FORD. Well, why did you not take a few million dollars to expand the ability rather than try to go, as we say down in west Kentucky, whole hog the other way?

Dr. DECKER. By "whole hog" you mean——

Senator FORD. Yes, you know, all of it into superconductivity when you take a few million dollars and accelerate it and get a lot more in return for the dollar spent from these other facilities?

Dr. DECKER. You mean to take some of the SSC money? Is that——

Senator FORD. Well, what is a few million dollars for a lot more productivity from the other facilities?

Dr. DECKER. Yes. As I said, the way the SSC budget was treated in the Department, it was held aside as really a totally separate issue, a separate pot of money.

Senator FORD. Well, it seems to me that the whole scientific community will be involved in this, and that all the facilities will be working together. And you said that Oak Ridge was one of the places where they would be working in this particular area.

Should we not fully utilize the facilities we have before we start building new ones?

Dr. DECKER. Well, that is a difficult tradeoff that we have to make constantly.

Clearly, within the budgets, if we were to put our resources on operating our existing facilities and not make commitments to new ones, then someplace down the road our scientists are going to be caught short in terms of having forefront capabilities.

So we need, as I said earlier, to try to strike some balance, which we try to do every year, between operation of existing facilities and starting new ones.

Senator FORD. Well, let us talk about priorities for money, and then this will be my last question. I do not see anybody here anxious to get involved in this heavy scientific work. And for a country boy, it is a little heavy to understand all of it.

If the Administration wants the SSC, there are two things in my opinion that just have to be done. First, we have to have some good rules for setting priorities among projects. What rules do you use, Doctor?

Dr. DECKER. That is a tough question. I think that, I struggled with this problem for a long time. I have been in the research and development business for, I guess, 20 years or so.

I think the best guideline really is to follow what are the best scientific opportunities, and that is what we really try to do. We try to determine which of the projects that come before us offer us the best opportunities to develop new knowledge.

Senator FORD. It seems like you are leaving that to us to do, rather than you doing it. I get the impression that we are having it thrown up here, and so we have got to make the decisions as to what is the best, what are the priorities among the various projects.

Dr. DECKER. I think there are a lot of proposed projects that are weeded out long before you get to see our budget request.

We do, of course, make extensive use of——

Senator FORD. You might be surprised. We might like some of the projects you have weeded out too.

Dr. DECKER. Well, that is possible.

Senator FORD. That is right.

Dr. DECKER. That is possible.

Senator FORD. So you get down to a certain point, and then throw them over here and say, you select them.

Dr. DECKER. I should point out that we do make use of a lot of advisory committees that are drawn from the scientific community to help us make those priority decisions.

We have used committees put together by the National Academy of Sciences. We have our own advisory committees, such as the Energy Research Advisory Board. We have advisory committees for each of our programs in the Office of Energy Research. And we do——

Senator FORD. Doctor, that is one of the reasons that we are making political decisions on where to put money as it relates to scientific work, new experiments, that sort of thing.

Because we find that every time you set up a committee you have a good old boy group, and it is the same group that gets together all the time, and they send the money to the same universities all the time.

That is the reason you find in Congress last time a valiant effort to see that some of the universities and colleges and other appropriate institutions had an opportunity to be involved.

Maybe it is proliferation. We find when you have these peer review groups, or these advisory committees, that money always goes to the same universities. And I went back for 14 years, and that is all I have been here, I did not think I had any right to say anything about the situation before that.

But ever since I have been here I can count the money and I can give you the 10 or 12 universities and institutions that get all the money, or basically all of it. So I worry about all this advice and counsel.

You never stray from that group, in my opinion, as it relates to where the money should go, because we know what is going to happen when the group gets together.

Let me just make one other—rather, I have two things. If you are going to have all this money, or all these new projects, or want to make decisions, the second point I wanted to make is that we must find some new sources of revenue available to science.

Do you have any ideas where we are going to get new sources of revenue?

Dr. DECKER. No, sir. With the Superconducting Super Collider we are trying hard to get foreign participation that would help offset the cost to the taxpayer. But in a general sense, no, I do not have a suggestion.

Senator FORD. So what you are saying is that we are asking for all this money and if we do not get it it is your fault? I am talking about Congress. And I understand where that comes from. It has been historical. Congress-bashing is not all bad for those in the Executive.

Let us go back to your advisory board just a minute. I seem to recall that you have an energy research advisory board, is that it?

Dr. DECKER. Yes, sir.

Senator FORD. What was their proposal to you or advice to the Department, their emphasis last year?

Dr. DECKER. Well, they did a number of studies for us.

Senator FORD. Let me help you out. They emphasized small science projects.

Dr. DECKER. Yes, they were very supportive of small science projects. And I think that they had the same recommendation as the Administration has with regard to Super Collider, in terms of not doing it at the expense of other areas of science.

Senator FORD. I do not have any other questions, Doctor. We appreciate you coming today. I think you begin to glean this Senator's attitude as it relates to the funding. And we all have parochial interests, and we all try to look at the big picture also.

And under the budget constraints, we have tried to make them fit. I am going to listen fairly closely to those that will be operating and building and responsible for putting it together, that is on the ground day by day.

You have the responsibility of maybe the overall picture. But I am going to be looking at those fellows and men and women who have been assigned the duty.

So we thank you, and we will be in touch, and I am sure we will have some interesting days before the President signs the documents that make up the budget.

Dr. DECKER. Undoubtedly. Thank you, Mr. Chairman.

Senator FORD. Yes, sir. Thank you, Doctor.

Dr. Leon Lederman.

Doctor, we are delighted to see you today, and you have a very short four-page statement and I am delighted with that, compared to 67 pages prior to you. So let us zero in on it and see it we cannot get after it here right quick this morning.

STATEMENT OF DR. LEON M. LEDERMAN, DIRECTOR, FERMI NATIONAL ACCELERATOR LABORATORY

Dr. LEDERMAN. Thank you, Mr. Chairman. I am the Director of the Fermi Laboratory, which has just completed the last stage of a 10-year program of improvements, the core of which is a 1,000 billion volt superconducting accelerator.

The TEVATRON, which we call it, is the highest energy particle accelerator in the world at this time, provides superb facilities for over 50 percent of the U.S. practitioners, largely coming from some 70 universities in the United States, and indeed for scientists from some 17 foreign countries.

We look forward to a decade of fruitful exploitation of this huge investment of creative man-years, and woman-years, and of public funds.

Our machine works. We have just completed a fantastic year-long period of operation in which our users have filled with excellent data an unprecedented 35,000 high-density magnetic tapes now stacked everywhere in our computer floor.

Mr. Chairman, laboratory directors are genetically incapable of being pleased with their budget, and I will omit the problems we have encountered in our intense desire to produce science with this new facility.

We are very proud of the fact that the TEVATRON is now considered a prototype for the Superconducting Super Collider. Indeed, were it not for our success, you would not now be burdened with these difficult decisions.

The SSC proposal obviously creates problems for the exploitation of the TEVATRON. However, as scientists who are concerned with the orderly evolution of our subject, and who are keenly aware of the SSC's awesome scientific potential, we believe a machine of SSC's power is a scientific imperative, and must appear before the end of the century, hopefully in 1997.

Through SSC, the United States has an opportunity to demonstrate boldness, imagination, and commitment to a quest for knowledge, which is the essence of what a great nation must include in its menu of essential activities.

Now, about fiscal year 1989. There are a large number of scientific initiatives proposed by the Administration in a year in which we are too, too aware of the pressure for deficit reduction. I do believe that many of these research initiatives are immensely valuable.

For example, I would like to see the Hubble Space Telescope in orbit, rather than collecting dust. And I would like to see the NSF's budget expanded to meet the needs of university-based research. Many of those small university groups are customers of our facility.

I obviously believe we must operate the completed new facility such as Fermilab's TEVATRON and the SLC machine at Stanford.

To do all these good things will require an increment in the annual basis research funding, and I estimate it at a few billion dollars a year, annually, added gradually over the next five years or so.

And I do believe this is the best investment this country can make in its future. The Senate Budget Committee actions indicated that many of you in the Senate share this view.

As for high energy physics, my lab, and I suspect the SSC, we can squeak through fiscal year 1989 with the level set by the Senate Budget Committee mark, which I suspect is somewhere around $150 million for SSC.

I would certainly like to see some of these funds designated for construction, so that the DOE can bring home the foreign commitments that Congress has so strongly urged.

Mr. Chairman, I would like to close with a few personal comments that are way above my job description but stem from some 40 years of teaching and doing scientific research.

I have lived through and experienced the post-war expansion of research and education, and the Sputnik effect, which made us roar through the 1950s and 1960s, the turnaround marked by the Vietnam War, the long slide of the 1970s, and the slow, painful recovery of the 1980s.

I have seen us fall further behind our economic competitors, and I have seen our projected national leads far surpass our ability to respond as we plunge towards the 21st century.

Any newspaper reader must be aware of a sense of failure of optimism in this country. And as only one example, a recent book predicting the decline of the United States is number two on the bestseller list.

This was best stated by one of your colleagues. A New York Times article of a few days ago quoted Senator Moynihan, in frustration over lack of support for a very advanced levitated train.

This is an American invention based on American technology, which is being pushed hard in Japan and Germany. Here is a Hitachi brochure on the dream train.

Senator Moynihan said:

> The problem is that nobody in this town thinks we do things like this anymore. For all the talk about competitiveness, we are now in a mindset that says we cannot afford to be first, we do not have the money, we do not have the means.

These remarks could equally well be applied to the basic research initiative, such as the SSC. And I can only pray for the sake of our grandchildren that it is not so. Thank you.

[The prepared statement of Dr. Lederman follows:]

Testimony of Leon M. Lederman
Fermi National Accelerator Lab
Committee on Energy and Natural Resources
Subcommittee on Energy Research & Development
United States Senate
April 12, 1988
Revised

Mr. Chairman, I am director of the Fermi Laboratory which has just completed the last stage of a 10-year program of improvements, the core of which is a one thousand billion volt superconducting accelerator. The TEVATRON, as we now call the modernized complex, is the highest energy particle accelerator in the world and provides superb facilities for over 50% of the U.S. practitioners (largely from some 70 U.S. universities) in high-energy physics and indeed, over 10% of the foreign scientists (from some 17 countries) working in this field. We look forward to a decade of fruitful exploitation of this huge investment in creative man-years of effort and of public funds.

Our machine works; we have just completed a fantastic, year-long period of operation in which our users have filled with new data an unprecedented 35,000 high density magnetic tapes which are stacked everywhere on our computer floor.

Mr. Chairman, Laboratory Directors are genetically incapable of being pleased with their budget and I will omit the problems we have encountered in our intense desire to produce science with our new machine. We are also very proud of the fact that the TEVATRON is now considered a prototype for the SSC, indeed, were it not for our success, you would not now be burdened with these difficult decisions.

As Fermilab scientists eager to exploit the TEVATRON, the SSC is a black cloud. Consider all possible outcomes: SSC fails to win congressional approval or, SSC goes to Illinois or, SSC goes elsewhere. Some thought indicates that each of those decisions creates problems for the exploitation of the TEVATRON. However, as scientists who are concerned with the orderly evolution of our subject and who are keenly aware of the SSC's simply awesome scientific potential, we believe a machine of SSC's power is a scientific imperative and must appear before the end-of-century, hopefully in 1997 or so.

Let me give you a particular vision of the SSC:

> Critics say the SSC is devoted to exotic research very far from the urgent human concerns that so obsess us today. This is true -- the concepts of quarks and grand unification are about as exotic as you can get, but listen -- 10,000 years ago, metals were not part of the human environment. They are rarely found in pure form in nature. After man found how to create them from ores, they played an increasingly important role in human affairs. Today we live in a metallic environment.
>
> Bulk electricity appears naturally only in lightning flashes and was studied out of curiosity as an exotic frontier research topic 150 years ago and today we live in an electrical environment which enriches and prolongs our lives.
>
> Prying into the nucleus 50 years ago was a purely academic activity directed towards advancing knowledge about the innermost structure of matter. Today medicine, biology, chemistry,

metallurgy are just some of the disciplines strongly influenced by nuclear science and technology. Also, fusion still holds promise of limitless energy with minimal waste products and no pollution. Nuclear phenomena have become an important part of our environment.

Now we are facing the next level -- the subnuclear domain -- the domain of discovery which requires the SSC. Can we now afford to turn away, to not build the SSC?

We can no more predict the societal benefits of SSC-derived fundamental knowledge than Bohr or Einstein could guess at the implications of their research into the atom. In view of this I find that the best reason for going ahead is that SSC is an essential tool in advancing our quest, handed down by generations of masters, for a comprehension of the universe in which we live. Through SSC, we in the U.S. have an opportunity to demonstrate the boldness, imagination and commitment to a quest for knowledge which is the essence of what a great nation must include in its menu of essential activities.

The issue I've been asked to address, I believe, is to make known proposals or alternatives for funding of basic research facilities for the 1989 fiscal year. I am aware of a large number of scientific initiatives proposed by the Administration in a year in which we are too, too aware of the pressure of deficit reduction. I do believe that many of these research initiatives are immensely valuable. Next to the high-energy physics, I'd like to see the Hubble Space Telescope in orbit and I'd like to see the NSF's budget

expanded to meet the needs of university-based research. I would like to see the nuclear facilities at CEBAF completed and the RHIC machine at Brookhaven started. I obviously believe we must operate the completed new facilities such as Fermilab's TEVATRON and the SLC machine at Stanford. I believe that the investment of an extra $2 billion dollars annually in basic research, added gradually over the next five years, is the best investment this country can make in its future. In zooming in to my field, I suspect SSC can survive in FY89 with a budget of about $150 million as suggested by the Senate Budget Committee mark-up. I'd like to see some of this designated for construction so that we can bring home the foreign commitments that have been so strongly urged on us.

A NY Times article of a few days ago quoted Senator Moynihan in his frustration at not generating support for an imaginative and very advanced levitated train. This is an American invention based on American technology which is being pushed hard in Japan and Germany. Senator Moynihan:

> "The problem is that nobody in this town thinks we do things like this anymore. For all the talk about competitiveness, we are now in a mindset that says we can't afford to be first, we don't have the money, we don't have the means."

These remarks could equally well be applied to the basic research initiatives such as SSC and I can only pray, for the sake of all of our grandchildren, that "it ain't so."

Senator FORD. I understand that. It is not so. Doctor, I tell you what I am going to do. We have Senator Domenici here now. But I am going to ask the other groups to come and visit with them, let them make their statements, and then we will ask you all to come back and we will kind of have a discussion among all of us.

So we will have the first panel, which consists of Dr. Fisher, Dr. Brinkman, and Dr. Sessler. Dr. Brinkman, Senator Bradley wanted to be here to welcome you and I do not know whether he is going to arrive or not. But we will go right ahead with this.

Dr. Fisher, where are you? You are in the middle. You are a next-door neighbor, if you want to start we will be delighted to hear your statement.

STATEMENT OF DR. MICHAEL E. FISHER, WILSON H. ELKINS PROFESSOR, INSTITUTE FOR PHYSICAL SCIENCE AND TECHNOLOGY, UNIVERSITY OF MARYLAND

Dr. FISHER. Mr. Chairman, Senator, I welcome the opportunity to speak and to address the issue of funding priorities.

Senator FORD. We are going in at 11 and start our budget on the Senate floor, and if you could abbreviate your statements I would be grateful. Not that I do not want to listen to your eloquence.

But it will be in the record, and if you would highlight that I would be grateful. Not that I would admonish you, I would admonish all three of you.

Dr. FISHER. Well frankly, Mr. Chairman, I have been impressed by the background, and I realize most of these arguments have been rehearsed before. To that extent, I am somewhat embarrassed to be here.

On the other hand, the reason I am here is because of my concern for small science. And if you will allow me, I would like to headline that.

Senator FORD. I am perfectly willing, and I am for you in that position.

Dr. FISHER. And then I would like to suggest that there are ways to prioritize that the scientists can do. It has been suggested that this is not possible. I think there are.

Dr. Cooper has a list of the priorities, and I would like to just mention those and try and briefly say how I personally feel they respond to this particular issue.

Just by way of introducing myself, I am something of a jack of all trades. I have worked in mathematics, chemistry, electronic engineering and various branches of physics, including some theory that has influenced high energy. And I have trained 60 young researchers who are now working broadly.

Big projects have their place in science and in technology. But in my view they are not its lifeblood. And the first point I would like to make is, well, who are the small science practitioners?

And as a rough and ready guide, if we take the American Physical Society, there are 34,000 members in the research divisions, 25 percent of the research membership is in the Division of Condensed Matter Physics.

That is small-scale physics. And if we include other small-scale physics in the APS, that brings the total of physicists roughly to two-thirds in small-scale physics.

The transistor and the semiconductors I do not have to tell you about. But I would like to mention that the last three Nobel Prizes in physics also stemmed from startling new discoveries in condensed matter physics. That is small science.

But in the United States today, small science is in serious danger. The Department of Energy is aware of that. As you yourself referred, Mr. Chairman, their own Committee over a year ago expressed major concern for the state of university-based research. And I think they were not exaggerating.

And if you would allow me, I would like to bring some news as it were from the trenches just to turn this into slightly more personal terms. I obviously have to just pick one symptom.

But let us look at the NSF Condensed Matter Theory Program. That is one of the areas that has a responsibility for supporting some of the thinking about the new "high-Tc" superconductors.

All previous funding commitments for the current fiscal year have been cut by a uniform 12 percent. Those are previous commitments, previous promises. In addition, grants are normally made for one year. These have been curtailed to seven or eight months.

Worse for our future well-being, it seems likely that this year only one single new junior faculty member in this area will be supported in the whole United States. And it is really superfluous for me to tell you the disheartening effect this has on other young men and women setting out on their careers, some of the brightest minds in their country.

The second largest division of the American Physical Society is particles and fields, is high energy physics. And indeed, 2,000 high energy physicists would benefit from the SSC.

In jest, some people have said, well, that leaves out the other 93 percent. And in honesty, I think most physicists support the scientific conception of the SSC, would look forward to learning what, if it is successful, it may have to teach us.

But the question is, should it be built? And should it be funded now? Surely the United States can afford a full spectrum of worthwhile scientific projects, including the Super Collider. But if sufficient funds will not be provided, how should priorities be set?

That is not an easy question, but I believe it can be addressed. And I would like to take as my text there some remarks of the President of the Royal Society in Great Britain, who has also faced the question of cuts.

And he has stressed the need for scientists to grasp the nettle and come to agreement about the relative values of different scientific projects. And I have proposed a list of criteria there which, Mr. Chairman, you have a copy of.

Senator FORD. Yes.

From the November 1987 Anniversary Address by the President of the Royal Society of London, Sir George Porter.

Discussing the need to grasp the nettle and come to agreement about the relative values of different projects in science he said:

"Some of the criteria we might use, in addition to the well established ones of timeliness and promise, are:

(i) the importance to other branches of science, of the knowledge being sought;

(ii) the ratio of the number of scientists involved to the cost of a project;

(iii) the urgency of the need for the new knowledge and whether it is likely that forthcoming developments may soon provide alternative, less expensive approaches;

(iv) if the proposal is fairly generally known, what special reason is there for us to take responsibility for it?

and most difficult, (v) a cost-benefit analysis, based on the likely improvement of natural knowledge, compared with the cost-benefit already pertaining in competing fields of science."

(emphasis added)

M. E. Fisher

Dr. FISHER. Briefly, they are the importance, he says in addition to timeliness and promise, which I think is not in debate here, the importance to other branches of science of the knowledge being sought.

The ratio of numbers of scientists involved to the cost of the project, and I personally would especially include those young scientists in training.

The urgency of the need for the new knowledge, and the question of whether it might be found in less costly ways. Is there any special reason why we should undertake the project? And most difficult, cost benefit analysis.

Now, I have tried to apply these criteria to the funding of the SSC, and I am going to be very brief and condense the arguments.

Firstly, it is true that the importance of one branch of science for another is hard to assess, it is hard to anticipate, even the historians. But to say that you cannot do anything, I think, overexaggerates.

And in my honest judgment, the extremely high energies and the tiny length scales to be probed by the SSC, fascinating though they are for many of us and I include myself there, are increasingly less relevant for other branches of physics and for those other sciences which operate on scales generally applicable here on earth, or even on the sun. And much the same must be said, I fear, when it comes to spinoff and the relevance of technology.

By contrast, some of the large projects already supported by the DOE, to which you referred, Mr. Chairman, represent important tools for a broad community of physicists and indeed for chemists, biologists, and others.

And personally, I would mention the reactor at Brookhaven, the Synchrotron light source at Stanford. That is my own personal knowledge. I am not trying to mention the others.

Now, the second criterion, ratio of scientists to cost, I have given some evidence. But I would like to highlight one future there. The future needs for educated manpower is well recognized in this country. Men and women.

And in a nutshell, 70 percent of the doctoral degrees in physics currently come from small science at a cost of less than 30 percent of the total Federal budget for basic physics, less than 30 percent.

Put another way, that means training a graduate student on a big science enterprise costs about six times as much as on a small research project.

And if I could interpose there again, such small research projects can be spread through many universities. The larger the facility the larger the concentration, the more it forces one to choose this area of the United States or another one.

I will leave the third and fourth points for my written testimony. They have been over before on this Committee. And I would like finally to address the cost benefit analysis.

I think everybody recognizes that cost benefit analysis is not yet a science. However, the subcommittee clearly knows that the proposed budget for the SSC, pro-rated over 10 years, is around, shall we say, $500 million per year.

It matches the current overall total fiscal year 1988 NSF budget for mathematics, for astronomy, for physics, for chemistry, and materials research. It matches that total.

A colleague of mine has observed that NSF's funding on small scale physics hardly amounts to more than one soft drink per inhabitant of the United States per year. That level of funding, frankly, is dangerously low.

So important for our future of scientific and industrial well-being, that fact cannot be ignored. It is clear that Congress wants to look broadly at the overall health and balance of basic research, and Mr. Chairman, I find myself embarrassed telling you this because that is clearly what you have said yourself.

But I want to stress again the importance of a panorama, a spread of small science projects. That is the lifeblood of science.

And finally, in these tight times, should not the Department of Energy accept a definite responsibility for picking up the small science ball that sadly the National Science Foundation has let drop.

I have tried to be brief. And I would be happy to expand my comments or answer any questions.

[The prepared statement of Dr. Fisher follows:]

TESTIMONY by MICHAEL E. FISHER
Cornell University and the University of Maryland
before
The Subcommittee on Energy Research and Development
United States Senate
12 April 1988

Mr. Chairman, Members of the Committee, I welcome this opportunity to speak to the Subcommittee and to address the issue of funding priorities.

My own interests in science and technology are broad: I have published research in mathematics, in computer science, in electronic engineering, in chemistry, and in various branches of physics — including work which has influenced high-energy theory. I have trained about 60 young researchers in these fields who are now working in academia, in industry, and in other professions. (A short 'profile' is appended.)

I am here today, primarily, because of my deep concern for the importance, at all levels of scientific and technological research, of the individual investigator interacting closely with one or two colleagues and a group of students and research associates.

Big projects certainly have their place in science and technology; but they are <u>not</u> its life blood.

At this time in the United States that life blood — "small science" — is suffering from a severe lack of oxygen and is in danger of being drained away, a situation that is surely of serious concern to this Subcommittee.

In the context of physics, what is "small science" and who are its practitioners? The American Physical Society (APS) has some 34,000 members affiliated with ten research divisions. The largest is the Division of Condensed Matter Physics representing more than 25% of the research membership:[1] here is a major home of small science. Other APS members, including those in the divisions of Atomic and Molecular Physics and of Chemical Physics, bring the small-science total to about two-thirds of the divisional membership.

Small-scale physics is a primary source of new ideas and discoveries. It is vital for the breadth of science. The story of the transistor and semiconductor electronics is well known; but it is worth mentioning that the last three Nobel Prizes in physics also stem from startling discoveries in condensed matter physics.

I have submitted a recent article by my colleague Professor Leo P. Kadanoff of the University of Chicago, which eloquently highlights the crucial role of small science in the context of present funding patterns.

But in the United States small science is in danger. The Department of Energy is certainly aware of the problem. Through its Basic Energy Sciences Program notable small-scale research is being supported. Yet, over a year ago, the Review Panel of DOE's Energy Advisory Board expressed "major concern" for university-based research, which it found to be in an "unsatisfactory condition:" the Panel was not exaggerating: on the contrary!

Please let me tell you some recent symptoms, as it were, "from the trenches." In the NSF Condensed Matter Theory Program — whose role is to support much of the thinking about the new "high-T_c" superconductors — all previous funding commitments for the current fiscal year have been cut by a uniform 12%. In addition, the normal grants of funds for one year have been curtailed to only 7 or 8 months. Worse for our future well-being, it seems likely that only one, single new junior faculty member in the United States will be given a grant this year. I need not tell you the disheartening effect news of this sort has on all those other young men and women scientists in the universities — some of the brightest minds in the country — as they start out on their careers.

On a larger front, in the NSF Division of Materials Research, yet another whole Materials Research Laboratory has been eliminated. But, in my view, the majority of these materials centers, spread across the country, have provided models of cross-disciplinary science and efficiently managed research investment.

One could go on; but the basic point is that small science in the United States is being starved.

The second largest division of the American Physical Society is the Division of Particles and Fields or "high-energy physics." It houses some 4600 members, about 13% of the total. In the main, this division represents "big physics": about 2300 high energy physicists in the United States would, indeed, benefit from the Superconducting Super Collider.

It has been remarked, in jest, that this leaves out the other 93%! Nevertheless, most physicists, I believe, support the scientific conception of the SSC and would look forward to learning what, if successful, it may have to teach us. But should it be built? Should it be funded now?

Surely the United States can afford a full spectrum of worthwhile scientific projects including the Supercollider. But, if sufficient funds will not be provided, how should priorities be set?

The problem is not easy but it can be addressed. Facing recent cuts in basic research in Great Britain — which have often seemed arbitrary and capricious — the President of the Royal Society of London, Dr. George Porter (a Nobel Laureate in Chemistry) has stressed the need for scientists to "grasp the nettle" and come to agreement about the relative values of different scientific projects. To this end he presented, last fall, a list of criteria to assist in decision making. He said:[2]

"In addition to the well established ones of timeliness and promise," we might use:

"(i) the importance to other branches of science, of the knowledge being sought;

(ii) the ratio of the number of scientists involved to the cost of a project" (and here I would especially include those young scientists under training);

"(iii) the urgency of the need for the new knowledge and whether it is likely that forthcoming developments may soon provide alternative, less costly approaches;

(iv) if the proposal is fairly generally known, what special reason is there for us to take responsibility for it?

and, most difficult, (v) a cost-benefit analysis, based on the likely improvement of natural knowledge, compared with the cost-benefit already pertaining in competing fields of science."

I have tried to apply these criteria to the issue of funding the SSC.

First, the "importance" of one branch of science for another, even within a single discipline, is subtle and not easily anticipated or unravelled; but in my honest judgement the extremely high energies and tiny length scales to be probed by the SSC, fascinating though they are to many of us, are increasingly less relevant for other branches of physics, and for those other sciences which operate on scales generally accessible on earth or even on the sun. Much the same must be said when it comes to "spinoff" and the relevance to technology.

By contrast, some of the large projects already supported by the DOE represent important tools for a broad community of physicists and, indeed, for chemists, biologists and others. As examples, I mention (primarily on the basis of my own personal familiarity) the high flux neutron beam reactor at Brookhaven National Laboratory and the synchrotron light source at Stanford.

The APS divisional statistics mentioned before provide some basis, within physics, for applying (together with DOE and NSF budget figures) the second criterion, the "ratio of scientists to cost."

However, since the future need for scientifically educated men and women in industry and defense is widely recognized, another statistic must be mentioned: in a nutshell, 70% of the doctoral degrees in physics now come from small science at a cost of less than 30% of the total federal budget for basic research in physics.[3] Put another way, training a graduate student on a big-science enterprise, costs about six times as much as on a small research project.

How about "urgency"? To most scientists, by their nature, their own projects are always urgent! Congress or, perhaps, the funding agencies collectively, need a broadly based scientific advisory system which could properly try to assess relative urgency within and across disciplines.

As to "alternative, less expensive approaches," others will testify: but the experts on machine design have certainly considered the issue carefully even though they have not convinced all their supporters. (A quote from a recent article by Professor Freeman Dyson of the Institute for Advanced Study is appended.)

Why should the United States take on the SSC alone? That question has also been raised before; but some of us, outside high-energy physics, have noted the successful multinational collaboration at CERN in Switzerland, and have been discouraged by the apparent reluctance of other scientifically advanced countries to join in on the SSC at an early stage.

Finally, "cost-benefit analysis" is not yet a science. However, this Subcommittee must know that the proposed budget for the SSC, prorated over ten years at nearly $500 million per year, matches the overall total FY88 NSF budget for Mathematics, Astronomy, Physics, Chemistry and Materials Research.[4]

A colleague of mine has observed that NSF spending on small-scale physics hardly amounts to more than one soft drink per year for each inhabitant of the United States! That dangerously low level of funding for small-science basic research — so important for our future scientific and industrial well-being — cannot be ignored. Congress, in setting priorities, needs to look broadly at the overall health and balance of basic research: we cannot afford to fund large enterprises, however glamorous, at the expense of a panorama of small projects — the life blood of science. In these tight times, should not the Department of Energy accept a definite responsibility to help pick-up the small-science ball which, sadly, NSF has let slip?

Mr. Chairman, I have tried to keep my testimony brief by condensing the arguments. I would be happy to amplify my remarks

and to answer any questions the Subcommittee may have. Thank you.

NOTES

1. The APS statistics here and below pertain to June 1987 (Executive Secretary's Report to the Council).

2. From the November 1987 Anniversary Address by the President of the Royal Society of London, Sir George Porter (Supplement to Royal Society News, vol. 4, No. 6, 1987.)

3. Figures drawn from Vol. 1, An Overview, of Physics through the 1990's (National Academy Press, Washington, D.C., 1986).

4. From budgets for the Mathematical and Physical Sciences Directorate of the NSF.

Pro^ile: Michael E. Fisher

Michael Fisher was born in Trinidad, West Indies and educated mainly in England gaining a Ph.D. from King's College, London University in 1957. Since 1966 he has been at Cornell University; in 1973 he was appointed as Horace White Professor of Chemistry, Physics and Mathematics; he served as Chairman of the Chemistry Department in 1975-78. At present he is on leave from Cornell as Wilson H. Elkins Professor in the Institute for Physical Science and Technology at the University of Maryland.

The theoretical researches for which Michael Fisher is known include the theory of critical phenomena and phase transitions, and the idea of dimensionality expansions, concepts which have influenced thinking in particle theory as well as in statistical physics, the behavior of polymers in chemistry, etc.

Dr. Fisher is a Fellow of the Royal Society of London and an Honorary Fellow of the Royal Society of Edinburgh. He was elected a Foreign Associate of the National Academy of Sciences in 1983. Among other honors, he received the Wolf Prize in Physics in 1980, and, in 1983, the IUPAP Boltzmann Medal, and the Luck award of the NAS.

Dr. Fisher has served on NSF, NRC and OTA Advisory Panels including the Committee for Review of Mathematical Sciences Funding in the U.S. chaired by Edward N. David. Currently he serves as a Councillor-at-Large of the American Physical Society.

REFERENCE FRAME

THE BIG, THE BAD AND THE BEAUTIFUL

Leo P. Kadanoff

This should be a good time for physics. Our President, several governors and many industrial leaders have eulogized science as significant both for its own sake and also in helping our nation maintain its long-term competitive position. But despite all this public goodwill, US physics continues to decay.

The first and most serious decay is on the industrial side. For example, Ed David estimates that over the last three years oil industry support of basic science and engineering has declined by about one-third. The breakup of AT&T has threatened the support base of the world's best industrial laboratory. Other labs have deteriorated, probably as part of the usual "life history" cycle of industrial laboratories. Because we have not seen any large offsetting growth of new private-sector centers in the physical sciences, the net result is decline.

Government-based science and technology have also done poorly. This result is most obvious in the NASA-based programs in planetary sciences, astronomy, atmospheric sciences and so on. As an apparent result of program misdirection, management incompetence and pursuit of publicity, during a period of several years we totally lost the capability of launching anything—shuttles or Atlases or balloons. So our once excellent planetary and space programs are in trouble. Still NASA continues to put publicity above science and technology. Corresponding losses have occurred in other areas of physics. Basic research in plasma physics has been too tightly coupled with fusion R&D, so that the recent cuts in the fusion program have hurt the basic work. "Environmental" science has disappeared precipitously from the national laboratories.

The universities have not done very well either. Undergraduate training suffers because high-school teaching in mathematics and physics is so very poor. An adequate supply of really good American graduate students is unavailable, so that graduate education (and the supply of teaching assistants needed to teach undergraduates) must rely upon the availability of students from abroad.

Another university difficulty is the spottiness of governmental support. There has been oversupport for some chosen areas (mostly based on big science) like supercomputer-based numerical computation or high power laser experiments. However, over the last seven years, we have seen attrition in support for small-group efforts. For example, condensed matter experiment, which is strongly based in the small-group mode, has had considerable difficulty in keeping sufficient support to remain competitive with analogous efforts in Europe or in industry. Additional cutbacks in Federal funding of condensed matter experiment are in progress.

The decline in the support for small science is particularly painful because the small-science mode is particularly appropriate for graduate student training. Students can plan small experiments, build the equipment, take the data and assess the connection of the output with theory. The time constants and the scale of large science are much less appropriate for training new scientists.

Governmental action to improve this situation seems to be largely misdirected. Instead of investing in small science our leaders seem to be entranced by less valuable but more arresting big projects: Star Wars before mine sweepers; billions for Superconducting Super Colliders, but inadequate funds to even run the neutron sources that might give insights into the new superconductors; money poured into supercomputer centers, but no support for the more economical use of local or individual computers on individual campuses; mathematics institutes funded, but a steep decline in support for faculty doing mathematics research; and so forth.

These tendencies can be seen in the NSF-sponsored Science and Technology Centers, which are intended to provide a meeting place for university and industrial scientists to work on common problems. The nation would indeed be helped by some carefully chosen additional possibilities for innovative research of the highest quality. Perhaps some of this would be helped by an interdisciplinary context in which new groupings of investigators would appear. But it looks as if these S&T centers are going to be a replacement for basic-science projects rather than a supplement to them.

These centers are also likely to focus upon the immediate needs of industry, rather than on basic science. In my view, this direction of development arises from a complete misunderstanding of what industry needs from the university world. By and large, big industrial concerns have sufficient resources to attack any technical problem they face. In some measure they need the universities to help in areas of sudden and unexpected need. For example, when oil firms suddenly realized that they could use tomography to "see into" rocks in a three-dimensional manner, they hired the knowledge of outside experts. But industry's most important need is for really new, "far out" ideas. Universities can serve as an important intellectual source. Big, project-directed science will be too focused and too goal oriented to provide the breadth and the innovation sorely needed by industry.

In this context, it is salutary to remember the disappointing results of the NSF Presidential Young Investigators Program. This program was intended to fund a group of promising young engineers and scientists and guide them into types of research that could be of long-term use to the US. The NSF was to give some money, and some of that money was to be matched

Leo P. Kadanoff, a condensed matter theorist, is John D. MacArthur Professor of Physics at the University of Chicago.

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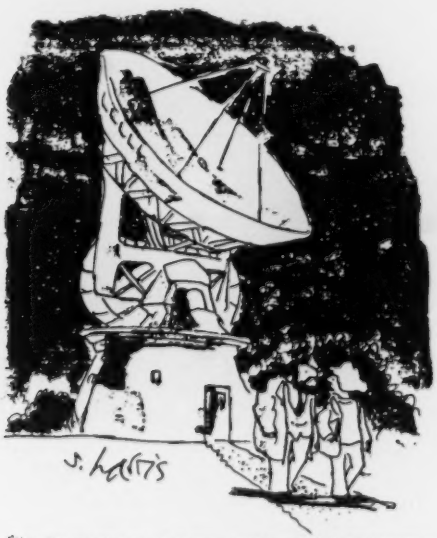
by industry, producing an incentive to joint exploration of new areas of interest. For many of the investigators, the match didn't work out. Often industry did not wish to invest in areas in which there would be only a long-term payoff—while promising young scientists did not wish to channel their research into areas with only short-term interest.

Much of the government sponsorship of research is forcing us to choose between the immediately fashionable and what will be interesting in the long term. The fashionable work is being structured into big projects and large teams, at considerable potential cost to the quality of US science and technology. Even in superconductivity, where almost all previous advances have come from very small groups, there is considerable governmental pressure for the construction of team-based "initiatives."

But I do not wish to suggest that the trouble lies entirely outside our physics community. In some measure, our own people are responsible. We scientists have chosen to follow fashion and have not continued research in less fashionable areas. We have thoughtlessly pushed our own subjects of research without concern for whether these areas have a value commensurate with their cost. And we have indeed pursued big science. While giving lip service to statements that a balanced and solid physics program requires a major investment in small science, physicists have pushed for each of the big projects. It is easier to argue for another synchrotron light source or for a supercomputer or supercollider than for the broad programs that will provide the real intellectual base for understanding our natural world. The big projects provide jobs. They have sufficient "sex appeal" and economic impact to command attention. However, in the long run an overinvestment in such efforts detracts from good science.

The true value of science is in the development of beautiful and powerful ideas. Overinvestment in big science detracts from what is really worthwhile. I do not think that the nation's or the government's budget for research or for R&D is too small. It is, however, increasingly misdirected toward grandiose projects. We physicists have a responsibility to understand what is truly valuable in science and use this understanding to help the nation develop and express its priorities. In doing this, we should push for the kinds of work that are of enduring intellectual and technical value, rather than projects that are "in the right field" or are simply easy to sell. ■



"I'LL BE WORKING ON THE LARGEST AND SMALLEST OBJECTS IN THE UNIVERSE—SUPERCLUSTERS AND NEUTRINOS. I'D LIKE YOU TO HANDLE EVERYTHING IN BETWEEN."

Extract from an article by

Freeman J. Dyson, Institute for Advanced Study, Princeton.

in Physics Today, February 1988.

Supporting particle physics but opposing the SSC:

"A prudent gambler places his bets so that no matter what happens, he stays in business. If a decision to build the SSC means that we give up the aggressive pursuit of laser acceleration and other innovative technologies, then the SSC may become as great a setback for particle physics as the space shuttle has been for space physics."

Dyson goes on to note that Dr. Andrew Sessler disagrees with him; see Dr. Sessler's article in Physics Today, January 1988.

Senator FORD. You have been very kind, Dr. Fisher. Thank you very much.

Dr. Brinkman.

STATEMENT OF DR. WILLIAM F. BRINKMAN, EXECUTIVE DIRECTOR, RESEARCH, PHYSICS DIVISION, AT&T BELL LABORATORY

Dr. BRINKMAN. Mr. Chairman, thank you for the opportunity to talk this morning on basic science and physics.

I come here partly because——

Senator FORD. Pull the microphone up just a little closer.

Dr. BRINKMAN. I come here partly because of my role as Chairman of the Physics Survey that wrote the volume "Physics in the 1990s" of two or three years ago, and would like to discuss my impressions of what has happened since that time.

Physics today is, as all sciences are, being pressured by two things. One of them, the budgets that you people are talking about. But the other is the fact that we have very much needed to enhance engineering and related subjects for our international competitiveness.

We have worked on that. That has had a big effect on the National Science Foundation, for example, where enhancement of those programs have been essential.

However, what happened is that the physics community as a whole is left with essentially very constant budgets, and yet what happened was the demands, the needs that we itemized back three years ago were much, much larger than the budgets we are currently getting, as you know.

If I look back at that survey and ask what things I thought were most important, I would say that at the time we looked at small science and we wrote about the fact that it was really in a troubled state.

We pointed out a number of things, instrumentation. We pointed out the decay of infrastructure of shops and electronics capabilities at universities, and a number of other things.

The fact that research costs have increased more rapidly than inflation, but whereas the budgets have not gone up that much. Those kinds of things were all pointed out, and we finally said that small science really needed, and physics, needed to have its budget doubled.

This really has not changed in three years. As far as I can see, if anything, the situation has gotten worse. The peer review process at the NSF has gotten closer to falling apart because of the fact that fewer proposals are being funded.

In addition, what is happening is Congress is clearly taking on its own initiative to fund things that are not being funded in the NSF. So both of those things are indicating that there are serious problems with funding of small science.

Finally, if you look at what is really happening in university faculties today you can see some evidence of problems. We in the survey looked at what we thought was going to be the average age of faculty members and were dismayed that this number is shooting up through the high 40s and heading into the 50s.

And we thought that things would start to get better, because the fact of the matter is that the faculties were all hired in the 1950s and early 1960s, after the war and just during the rapid expansion of science in the early 1960s.

We thought those retirements would come in. What is happening in fact, however, is that the average, the median age of university faculty is actually increasing faster than we anticipated, which we believe indicates that, in fact, young people are not being hired as rapidly as we expected.

The other thing that we found, what I have found, and a survey of university physics department chairmen has also found, is that there is a dearth of candidates to be hired in condensed matter physics and related fields. So that in fact we are undersupplied in condensed matter physics at this stage.

I find that at Bell Laboratories where we consistently hire in that area. Therefore, I believe that it is quite clear that there are serious problems in university research that need to be addressed.

And I believe the best way to address that is to increase the funding of the NSF, as proposed by the Administration, and to increase the funding of basic energy sciences in the DOE.

Finally, you might ask what has that to do with the SSC? But the fact of the matter is, you have already pointed out, we have to establish priorities. And what I am saying is that I believe that that is the most important priority in this country right now in science, and that we must move forward with that.

We would all love to see the SSC funded, but I think we have got to give the first priority to straighten out the university situation.

As a last item, I would like to mention this issue of priorities. And I want to make clear what was done in the physics survey and what I think further can be done.

When the physics survey was done three or four years ago, the view was that we have a set of advisory priority-setting committees for high energy physics. We have the High Energy Physics Advisory Committee, for the nuclear physics we have the Nuclear Sciences Advisory Committee, et cetera, et cetera.

These committees are able to set priorities because the view is that the goals of that group of people are sufficiently similar that you can define the scientific goals well enough to do a scientific job of prioritizing.

As Michael Fisher has already pointed out, if you want to do prioritizing among the subfields, you must then look at a broader spectrum of issues. And that is what we, the physics community had said we would like to leave that up to the political process as long as possible.

However, as these budgets have gotten more difficult to deal with, it is clear that we can do more. A number of us are urging the physics community to start thinking about doing more, and to try to work with the DOE and the NSF in trying to understand how we might move forward on setting priorities between subfields of physics.

So I think that that is possible to do, and I think the physics community is starting to grasp and work on that issue. Thank you very much.

[The prepared statement of Dr. Brinkman follows:]

TESTIMONY OF
W. F. BRINKMAN
AT&T BELL LABORATORIES

Senator Ford and members of the Senate Subcommittee on Energy Research and Development, thank you for the opportunity to express my views on the subject of funding of basic science in physics and, in particular, the Superconducting Super Collider.

I am here today not as a representative of industrial research, but rather to present my views on physics funding after having served as Chairman of the National Academy of Sciences Report "Physics into the 1990's" three to four years ago. In discussing this subject, I will not restrict myself to the Department of Energy funding, but rather the overall funding of physics.

Science funding, in general, is being strongly impacted by two forces. The first is the relatively constant budgets brought on by the national deficit, and the second is the increasing awareness of the need for international competitiveness in high technology. The first tends to keep our budgets fixed, while the second tends to draw the money available away from basic scientific research toward engineering and related subjects. These two pressures have led to a situation in physics where the funds required to satisfy the needs identified by the community far exceed the funds available. A summary of these

needs was put together by the American Physical Society's Panel on Public Affairs, and is being presented here by Dr. A. Sessler.

One of the facilities in this list is the Superconducting Super Collider for which the costs are considerably larger than all the other projects. For this reason, it attracts a lot of attention.

There are a number of important considerations that need to be taken into account in deciding what are the most important activities to fund in physics. The funding of university research in small groups has remained at a relatively constant and unhealthy level over the past several years since the physics study was completed. In the report, we said "evidence in the panel reports (reports from various subfields) indicates that the climate in which university research takes place is generally troubled". We then said that, "When one views small-group research collectively and witnesses its enormous effect on science education and our universities, as well as the abundant returns it yields to society, the endeavor surely represents one of the most important ways for the nation to invest in research". From these remarks, and other evidence, we concluded that funding for individuals and small groups doing university research needed to be doubled. However, not much has changed in the intervening years. The result is a situation where only a few new proposals are funded each year and where the funding has focused on fewer institutions. As a result, the peer review system is on the verge of not working and Congress finds

itself increasingly directly funding specific proposals. Excellent science is performed by the university research community in all the subfields of physics, but particularly in Condensed Matter Physics. Atomic and Molecular Physics, and related subjects such as Materials Science. This basic research is very important for advancing new scientific concepts such as chaos, neural networks and superconductivity that are at the intellectual forefront. At the same time, it is absolutely essential to the future technological development of this country. For these reasons, it must have the highest priority.

In our universities, the physics faculties were largely populated in the 1950s and early 1960s. These post-war scientists are now beginning to retire. Two important issues arise as a consequence. First, are there sufficient candidates to fill these jobs, and the second is the funding situation for university research attractive to young candidates? I believe the answer to both questions is no. Several bits of evidence indicate this. The mean age of physics department faculties is increasing faster now than we predicted a few years ago* indicating to us that those who are retiring are not being replaced as fast as we thought. University physics department chairmen surveyed claim they cannot find sufficient candidates to fill positions in fields like experimental condensed matter physics.* We, that is AT&T Bell Laboratories, are finding this to be true

* Information supplied by Beverly Porter of the American Institute of Physics.

as well. Secondly, although universities have very substantially increased their salary offers and start-up monies for new faculty, young scientists that I know are still struggling to get federal grants. We should not delude ourselves on this matter. It is entirely appropriate that the Federal Government supply the majority of research money; thus, while the universities can put together start-up monies they must rely on Federal funds.

Since universities are the lifeblood of science in this country, no matter where it is performed in industry, at the National laboratories or at the universities themselves, the highest priority in funding surely must be to fund university research. Therefore, I strongly encourage support of the National Science Foundation budget and the program to double that budget over five years. University science needs that support and so does the engineering research programs that are closely related in international competitiveness.

We would all like to see enormous increases in the 250 budget function. Since that isn't likely, we must make choices; and my first choice would be to take care of university research and after that consider the SSC. The SSC is a well thought out project that will explore a new region of energy and new discoveries may well be found. However, it will do nothing to alleviate the critical problems cited above.

You also asked us to make suggestions on establishing funding priorities for DOE basic science facilities. The physics community has

established procedures for setting priorities within the various subfields of physics -- elementary particle physics uses the High Energy Physics Advisory Committee, the nuclear physics community has the Nuclear Science Advisory Committee, and so on. We have not gone beyond that structure because the various subfields have well-defined scientific goals, and priorities can be set within the context of those goals on scientific and technical grounds. For example, it is difficult on purely scientific grounds alone to decide whether it's more important to build a machine to find the next elementary particle versus one that will allow a large number of surface structures of solids to be determined. Issues that would come into that decision involve questions not only of the intellectual content of the work, but also the relevance of the work to other fields (for example, x-rays from synchrotrons are increasingly being used by molecular biologists to determine structures of protein) and to future technology. Such choices, therefore, depend on issues that have to do with societal values as a whole. So far, physicists have chosen to allow the political process to decide these issues rather than have a committee of wise men. We, of course, try to participate in this process. Today, as the reality of the budgets tends to squeeze on the needs and opportunities defined by the various subfields, we are becoming aware that we should consider more closely the overall needs of physics and see if we can deal with the issues involved. Considerable discussion is occurring in the community. It is something that we need to work

out with the Executive Branch, particularly the DOE and NSF. There are minimal actions that can be taken, such as the examination of the possibility of combining facilities, and making better use of existing facilities and these should be considered. Since there are also risks involved, thoughtful planning is essential. This needs to be done within the community of physicists, and hopefully will be endorsed by the DOE and NSF.

I hope these comments are of use to you in considering your decisions, and I thank you for the opportunity to present my views.

Senator FORD. Thank you very much, Dr. Brinkman.
Dr. Sessler.

STATEMENT OF DR. ANDREW M. SESSLER, ACCELERATOR PHYSICIST, LAWRENCE BERKELEY LABORATORY

Dr. SESSLER. Mr. Chairman, thank you for the opportunity to testify.

I am from the Lawrence Berkeley Laboratory in California. I am an accelerator physicist who has worked through the years on the theoretical ideas which are employed in the design of high energy physics accelerators.

For a seven-year period I was director of the Lawrence Berkeley Laboratory in which position I had the responsibility of managing and directing a major facility. This year I am chairman of the American Physical Society Panel on Public Affairs. The opinions I wish to express are, of course, my own.

I would like in this testimony to first comment upon the state of technology and the SSC, and then second I would want to comment upon the opportunities and fiscal needs of physics and the SSC.

First, technology. The SSC is at the frontier of technology. It proposes to employ advanced techniques, but I have every reason to believe it can be made to work as predicted.

The history of accelerators is a stellar history in which time and again machines have been built on time and on budget. I believe that the SSC would properly fit into this tradition.

Could new technologies, such as the high temperature superconductors, be used in the SSC? I do not believe so. Certainly not for a decade or more, and possibly never. You know the detailed arguments. I need not go into them here. But I want to make clear that I find these arguments compelling.

Could novel acceleration techniques be employed to accomplish what the SSC seeks to do, and do it either better or do it for less cost? If we examine the new techniques, we find that none are ready to be incorporated into a machine at this time.

At least five years, and possibly 10, of intense R&D is needed before these new methods are available for use in an electron positron, as contrasted with a proton collider. Once the new techniques are available, it will take another five years or so to construct a machine.

Finally, such a machine will not be cheap. An accelerator capable of studying the same physics as the SSC would cost billions of dollars.

Second, I would like to turn to physics needs. Physics basic research has received essentially the same amount of dollars per year over the last decade. The results have been very serious.

It does not matter if you examine teaching facilities in universities, look at small research, or study large laboratories. The conclusion is always the same. The signs of inadequate funding are everywhere to be seen.

Poor, old, often non-functioning teaching and training equipment. Distinguished researchers on, let us say, superconductivity having their meager support reduced even this year.

Large accelerators typically being operated only 60 percent of optimal utilization time in order to save on the electric power bill. To remedy this situation will require increased support of basic physics.

I have reviewed the carefully considered needs in condensed matter science, plasma physics, nuclear physics, and high energy physics.

First, major new facilities whose earliest possible construction is strongly desired by the community are very much needed, in my opinion.

Second, I believe that we also need growth in small science. Small science supplies, as has just been pointed out, approximately 70 percent of the Ph.D.s, and 70 percent therefore of employment of physicists.

As you can see in this figure, Figure 2 in my testimony, the sum of the needs, these needs, requires more than a factor of two increase in the physics funding.

These numbers are preliminary. It is only because of the timing of these hearings that they have been released. We are still working on them. But for our purposes today they quite adequately serve.

Senator DOMENICI. Mr. Chairman, might I ask a question?

Senator FORD. I always recognize seniority and good looks.

Senator DOMENICI. What does small physics mean as you use it there?

Dr. SESSLER. It means, we have defined it this way, but it is approximately true, sort of like 12 percent of high energy physics, 12 percent of nuclear physics, approximately 25 percent of plasma physics, 100 percent of condensed matter physics, atomic optical physics, things like that, 100 percent of those.

Senator DOMENICI. That is the definition of small physics?

Dr. SESSLER. That is right. That is what I used when I made up these figures.

Senator DOMENICI. Can I interject chemical physics?

Dr. SESSLER. Oh, sure, there are many other things, but I am giving a rough idea.

Senator FORD. The university type.

Dr. SESSLER. Generally, this is at universities.

Dr. BRINKMAN. But the DOE does a fair bit of small research at a variety of its laboratories. At Argonne National Laboratory, or Oak Ridge, or Sandia, there is a fair bit of small research that is done in those laboratories.

Dr. SESSLER. To repeat then, this graph shows that a factor of two in real dollars is needed to adequately support physics.

Notice that 1976, where the figure starts, was actually a minimum in support. And in 1988 we are only back to where we were in 1967.

I would like now to turn to the question of SSC funding. I do not believe we want to tell a high energy physicist to build something other than the SSC.

It is the overwhelming view of that community that the SSC is a device of choice. And as I have said, alternative devices cannot be built at this time. The arguments for getting on with it you have heard. I need not repeat them.

Should the SSC be authorized? In order to answer that question, I must ask another question. In what context? I can imagine a number of different contexts. First of all, if there is a general increase by about a factor of two in physics funding, then of course the SSC should be approved.

Note that this requires an increase in operating funds for high energy physics from $530 million to $800 million in fiscal year 1988 dollars.

An alternative context is one in which funding of the SSC would have the consequence of reducing the funding of other branches of physics. Even the high energy physicists have said that they do not desire this outcome.

I would not support the SSC if I thought the consequence would be a reduction in funding of other branches of physics.

Third, another possible context, a more likely one is somewhere between these two alternatives. Perhaps if the SSC is given the green light, then the increased funding of high energy physics would raise the ante, and result in due time in increased funding of other fields of physics, a kind of trickle-down effect if I may use that term.

One argument goes that if the SSC is not funded, the money will not go to other scientific projects. And since something is better than nothing, all scientists should support the SSC.

We must note, however, the deep concern on the part of most physicists that approval of the SSC will be still another initiation of a very noticeable, glamorous, big-science project without adequate appreciation of the importance and support of small science.

Even though I believe this trickle-down theory, I think that need for support now in all areas of physics is so important that I would not support SSC approval in this context.

Still another context is that in which approval of the SSC will come with a modest increase of support across the board to all of physics.

If the Congress initiates the SSC this year, and then a nuclear science project, like RHIC, next year, et cetera, combined with a commensurate increase in small science, that is an increase in operating funds for basic physics and DOE and NSF over a number of years, then I would support the SSC.

Finally, then, in conclusion, a piecemeal approach of approving only SSC and doing nothing else for physics would be ill-advised, and in fact divisive for all of physics.

In my opinion, the Congress should approve the SSC but couple this approval with increased support of the basic physics program, that is more operating funds, and a systematic program of funding other important physics facilities, additional construction funds.

It is this multifaceted, multiyear approach which is needed in order to restore the strength and vitality of U.S. physics.

Thank you. I would be delighted to attempt to answer any questions.

[The prepared statement of Dr. Sessler follows:]

Testimony on the SSC before the U. S. Senate Committee on Energy and Natural Resources and Development;
Subcommittee on Energy Research and Development,
on April 12, 1988.

I. Introduction

Thank you for the opportunity to testify. I am Dr. Andrew M. Sessler from the Lawrence Berkeley Laboratory in California. I am an accelerator physicist who has worked through the years on the theoretical ideas which are employed in the design of high energy physics accelerators. For a seven year period, 1973-1980, I was director of the Lawrence Berkeley Laboratory (LBL) in which position I had the responsibility of managing and directing a facility with a budget of over 100M$ per year. This year I am chairman of the American Physical Society (APS) Panel on Public Affairs. (I have supplied the committee staff with a short curriculum vitae.) The opinions expressed herein are, of course, my own and are not necessarily those of LBL or the APS.

I would like in this testimony to first comment upon the state of technology and the SSC. Second, I want to comment upon the opportunities and fiscal needs of physics and the SSC.

II. Technology

For the last 5 years or so, I have worked on novel acceleration techniques investigating methods that would be used in machines beyond the SSC, namely on large electron-positron (as contrasted with proton) colliders. Some of these new methods I have described in a recent article in Physics Today, a copy of which I have supplied to the committee staff.

Let me say that in my view the SSC is at the frontier of technology. It proposes to employ advanced techniques, but I have every reason to believe it can be made to work as predicted. The history of accelerators is a stellar history in which time and again machines have been built "on time" and "on budget". I believe that the SSC could properly fit into this tradition.

Could new technologies such as the high temperature superconductors be used in the SSC? I don't believe so; certainly not for a decade or more and possibly never. You know the detailed arguments; I needn't go into them here, but I want to make clear that I find these arguments compelling.

Could novel acceleration techniques be employed to accomplish what the SSC seeks to do and do it for less cost? If we examine the new techniques we discover that none are ready to be incorporated into a machine at this time. At least 5 years, and possibly 10 years, of intense R&D is needed before these new methods are available. (I am devoting years of my life to the subject and I am optimistic that the R&D will "pay off".) Once the new techniques are available, it will take another 5 years or so to construct a machine. Finally, such a machine will not be cheap: an accelerator capable of studying the same physics as the SSC would cost billions of dollars.

III. Physics Opportunities and Needs

Physics research has received essentially the same amount of dollars per year over the last decade. The result has been very serious. It doesn't matter if you examine teaching facilities in universities, look at small research, or study large laboratories and their associated facilities, the conclusion is always the same. The signs of inadequate funding are everywhere to be seen: poor, old, often non-functioning teaching and training equipment; distinguished researchers on (say) superconductivity having their meagre support reduced (even this year!); large accelerators typically being operated only 60% of optimal utilization-time in order to save on the electric power bill.

To remedy this situation will require increased support of physics. In the attached graphs, based on work by the Panel on Public Affairs of the American Physical Society, I have indicated the carefully considered needs in Condensed Matter Science, Plasma Physics, Nuclear Physics, and High Energy Physics. (See Fig. 1.) In the Table I have indicated the major new facilities whose earliest possible construction is strongly desired by the community and very much needed in my opinion. The figures include these new facilities as well as a growth in "small science".

You can see that the sum of these needs requires more than a factor of two increase in the funding of physics research, as is shown in constant dollars in Fig. 2, and then in current dollars in Fig. 3.

IV. SSC Funding

I believe we don't want to tell the high energy physicists to build something other than the SSC. It is the overwhelming view of that community that the SSC is the device of choice and, as I have said, alternative devices can't be built at this time. The arguments for "getting on with it" you have heard, I need not repeat them. They have to do with everything from training of new people, through not losing the accumulated expertise in this generation, to the need, excellence, importance, and excitement of the science.

Of course all of the above arguments can be made by the scientists in other branches of physics. And those arguments are equally valid and, I hope, equally compelling. As I have said, a large increase in funding for all physics is needed.

Should the SSC be authorized? In order to answer that question, I must ask another question: In what context? I can imagine a number of different "contexts":

1) If there is a general increase -- by about a factor of two -- in physics funding then, of course, the SSC should be approved. (Note that this requires not only construction funds, but -- even with economies being made in other aspects of high energy physics -- an increase in operating funds for high energy physics from 532 M$/yr to 800 M$/year. [FY'88 dollars])

2) An alternative context is one in which funding of the SSC would have the consequence of reducing the funding in other branches of physics. This possibility causes deep concern amongst all physicists. Even the high energy physicists have stated that they don't desire this outcome. I would not support the SSC if I thought the consequence would be a reduction in funding of other branches of physics.

3) Very likely, the appropriate context is somewhere between these two alternatives. Perhaps, if the SSC is given "the green light" then the increased funding in high energy physics would "raise the ante" and result, in due time, in increased funding in other fields of physics. (A kind of "trickle down" effect.) We must note, however, the deep concern on the part of most physicists that approval of the SSC will be still another initiation of a very noticeable, glamorous "big science" project without adequate appreciation of the importance and support of "small science". Even though this "trickle down" effect may be real, I believe the need for visible support in all areas of physics is so important that I would not support SSC approval in this context.

4) Still another context is that in which approval of the SSC will come with a modest increase of support -- across the board -- to all of physics. One could plan on initiating the SSC this year, then a nuclear science project (e.g. RHIC) next year, etc. Such a program needs to be combined with a commensurate increase in "small science"; i.e. in an increase in operating funds for basic physics in the DOE and NSF. Physics is in need of both large facilities and increased funding of the base program. If the Congress would couple approval of the SSC with an increase in operating funds for the base program then I would support approval of the SSC.

V. Conclusion

A "piecemeal" approach of approving only the SSC and doing nothing else for physics would be ill advised and in fact divisive for physics. In my opinion the Congress should approve the SSC but couple this approval with increased support

of the basic physics program (i.e. more operating funds) and a systematic program of funding other important physics facilities (additional construction funds). It is this multi-faceted approach which is needed to restore the strength and vitality of US physics, thus building a solid foundation upon which can rest a superstructure of US economic well being and military strength.

TABLE I. MAJOR NEW FACILITIES

	TOTAL COSTS
HIGH ENERGY PHYSICS	
SUPERCONDUCTING SUPER COLLIDER The SSC would supply colliding proton beams at 40 TeV, the highest energies proposed to date. It would allow the search for heavier particles, to answer the question of what generates mass and to test new theoretical ideas about the fundamental nature of matter.	$3.2B
NUCLEAR PHYSICS	
CONTINUOUS ELECTRON BEAM ACCELERATOR FACILITY (CEBAF) CEBAF is a 4-GeV linear-accelerator stretcher-ring complex to investigate the microscopic quark-gluon aspects of nuclear matter.	$225.OM
RELATIVISTIC HEAVY ION COLLIDER (RHIC) RHIC is a mahine that would accelerate heavy ions to 10's of GeV per nucleon. The purpose of RHIC is to investigate the possibilities of quark-gluon plasma high density nuclear matter.	$325.OM
CONDENSED MATTER PHYSICS	
1-2 GeV SYNCHROTRON This Synchrotron is designed for insertion devices to produce high brightness sources of radiation in the ultraviolet and soft X-ray region of the spectrum to explore the electronic structure of matter.	$98.OM
6-7 GeV SYNCHROTRON This Synchrotron is optimized to create extremely high brightness sources of X-ray radiation for exploration of the structure of surfaces, interface biological molecules and other materials.	$385.OM
HIGH FLUX REACTOR A new high flux reactor would increase the neutron flux for various materials science experiments by over an order of magnitude. Neutron scattering is essential for understanding excitations of matter in regions not accessible by other techniques.	$300.OM
PLASMA PHYSICS	
COMPACT IGNITION TOKAMAK (CIT) The next step in magnetic fusion is exploring the behavior of a fusion ignited confined plasma. The CIT would allow studies of the behavior of such plasmas and would act as an intermediate step to a fusion reactor.	$357.OM

Fig. 1 Projected funding needs for four subfields of physics in the Department of Energy, in current dollars. (a) High Energy Physics, (b) Nuclear Physics, (c) Condensed Matter Physics, and (d) Plasma Physics. No increases in the operations budgets have been included other than 3% inflation per year.

Fig. 2 A compilation of physics opportunities and needs. The growth in "small physics" is taken to be a doubling in 5 years. The facilities are those shown in Fig. 1.

Fig. 3 The same as Fig. 2, but with projected inflation averaging 6% per year for construction and 3% for operations.

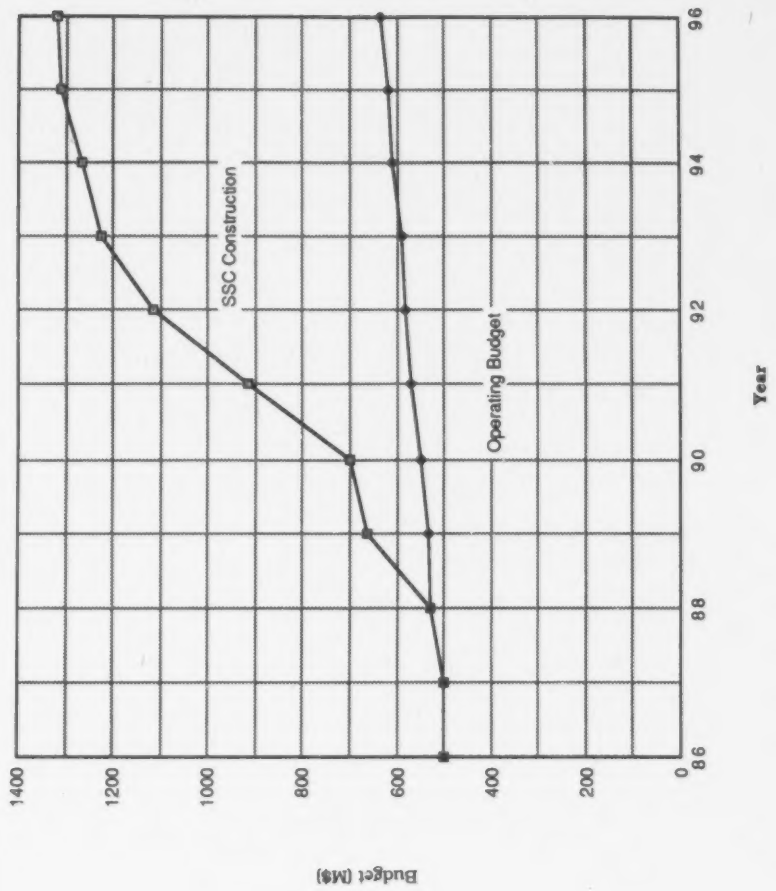


Fig. 1a: High Energy Physics

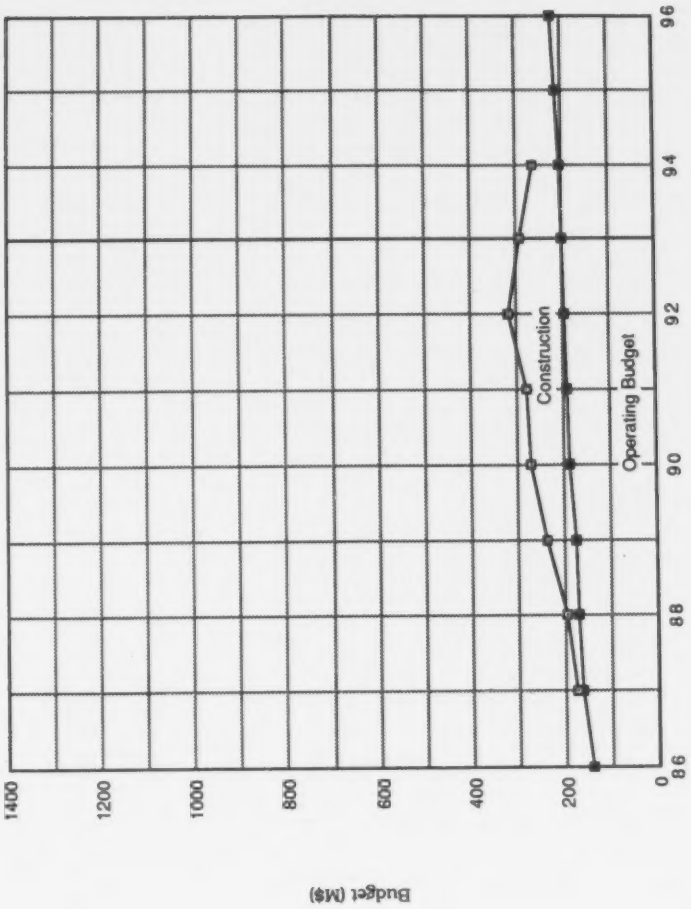
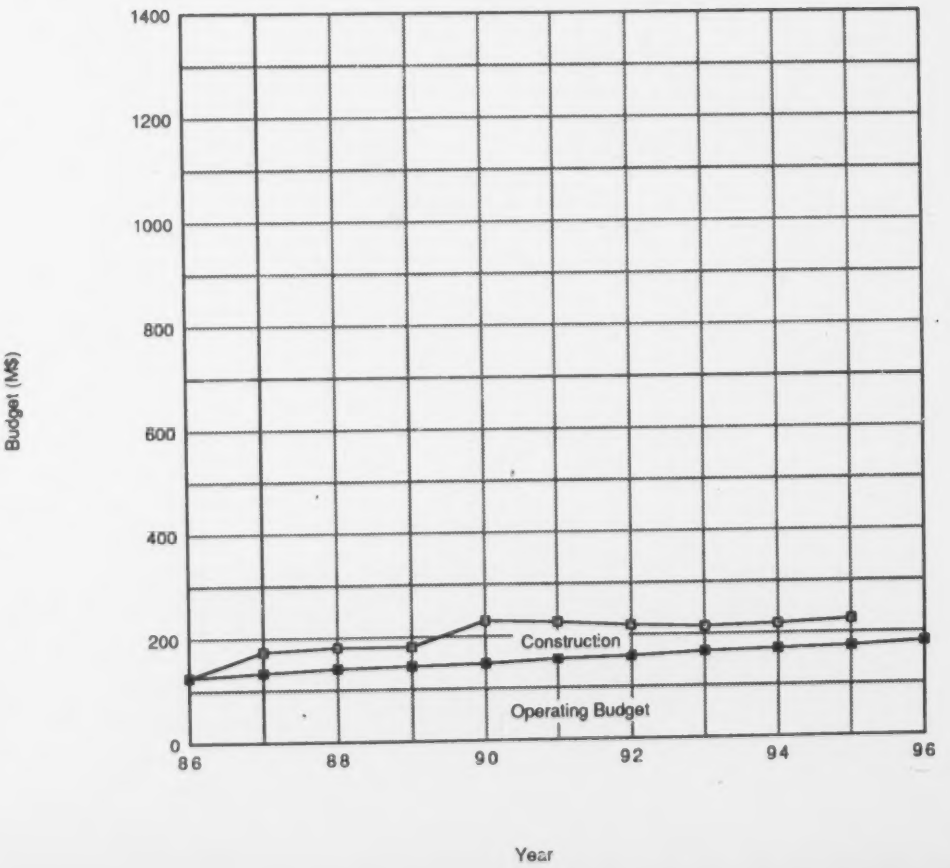


Fig. 1b: Nuclear Physics

Fig. 1c: Condensed Matter Physics



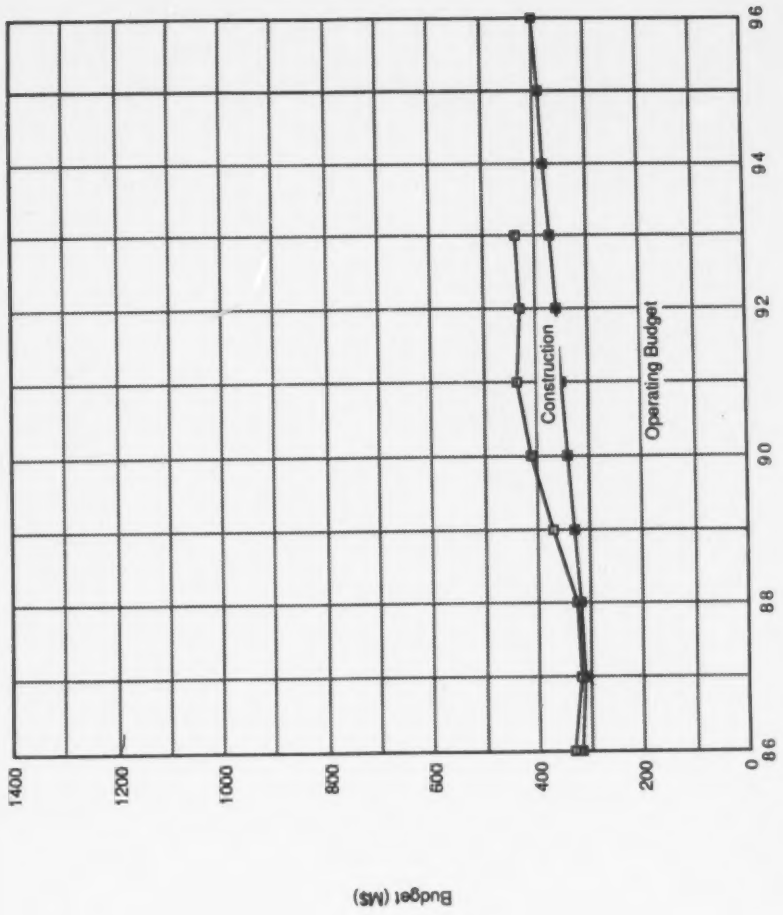


Fig. 1d: Plasma Physics

Fig. 2: Total Physics Funding In Constant 1988 Dollars

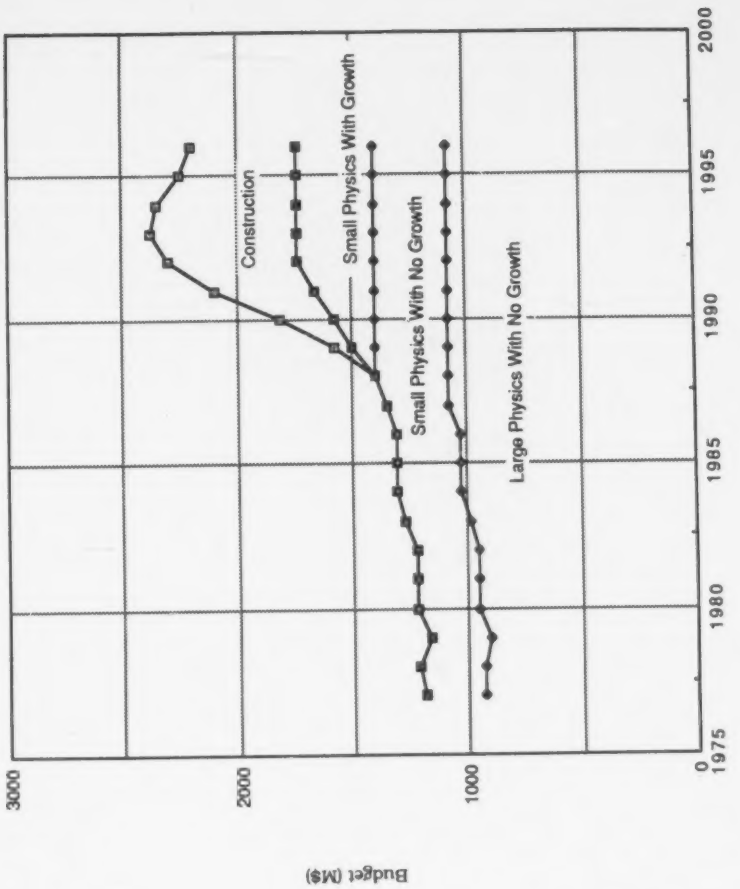
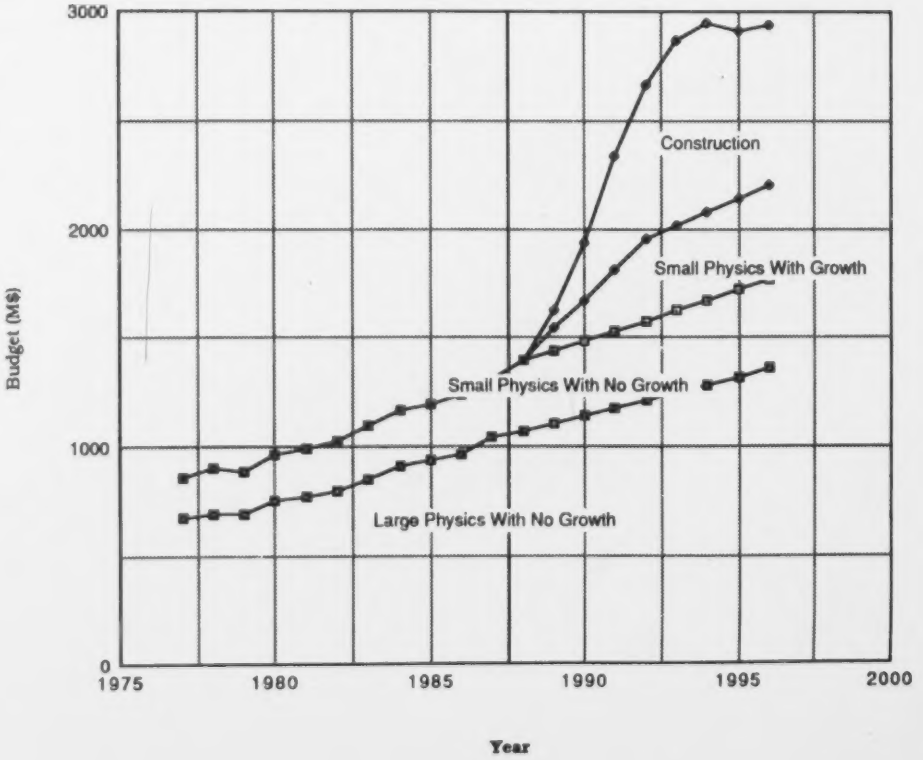


Fig. 3: Total Physics Funding In Current Dollars



NEW PARTICLE ACCELERATION TECHNIQUES

The next generation of electron–positron colliders will have to be linacs with very large accelerating gradients provided by more or less novel techniques.

Andrew M. Sessler

In 1932 John Cockcroft and Ernest Walton developed an electrostatic accelerator at the Cavendish Laboratory, thus starting the modern age of particle accelerators. Since then, our capabilities have increased tremendously, as may be seen in figure 1, which shows the energy of accelerators through the years. There has been an exponential increase in energy (the "Livingston curve")—but it is the envelope that displays this exponential behavior, not any particular technique. One can conclude that if we are to stay anywhere near the Livingston curve, new techniques need to be developed.

I shall describe some of the new techniques that will be employed as we build accelerators even more powerful than the colliders just completed (the Tevatron collider at Fermilab, Tristan at KEK in Japan and the Stanford Linear Collider), those under construction (LEP at CERN, HERA in Hamburg and UNK at Serpukhov) and those under design (SSC in the US and the Large Hadron Collider in the LEP tunnel).

Circular accelerators are already remarkably effective. In fact, no one has any ideas for improving them, other than in an evolutionary manner. For example, new superconductors may provide stronger bending magnets, or less demanding cryogenic requirements. (If the new high-temperature superconductors are to be useful for accelerators, they will require high critical current density and the material properties that will permit the formation of wire.) Needless to say, such evolutionary changes can be very significant.

For protons, even at beam energies very much greater than that of the 20-TeV SSC, circular accelerators are quite adequate. Not so for electrons, where synchrotron radiation forces one to use linear machines. At 1 TeV, for example, an electron in the 50-mile-circumference SSC tunnel would radiate away more than half its energy in just one turn! Because center-of-mass energy increases only as the square root of beam energy in a fixed-target relativistic accelerator, all modern accelerators are colliding-beam devices. Thus, all of the research efforts on new acceleration techniques for high-energy physics are focused on linear electron colliders, and here I will consider only such schemes.

The Stanford Linear Collider will soon be providing experimenters with 100-GeV e^+e^- collisions. The two-mile-long linear accelerator that speeds these particles up to 50 GeV provides an accelerating gradient of 17 megavolts per meter—more or less the present state of the art for rf linacs. If we are to have TeV linear e^+e^- colliders of reasonable length, we will need an order of magnitude increase in accelerating gradients. We do not yet have such a capability in hand. Intense efforts are now underway at CERN, SLAC, KEK and Serpukhov to develop the techniques that would allow these labs to build TeV linear colliders early in the 1990s.

We will discuss only one aspect of such colliders—the acceleration process itself. The reader interested in other important aspects of linear colliders, such as damping rings, focusing systems and the disruptive interaction of the beams at collision, should consult the literature.[1] A number of recent conferences have been devoted to the subject of novel acceleration schemes, and the reader seeking more detail than can be included here may wish to consult their proceedings.[2–6]

Categorizing accelerators

Particle accelerators do their work by means of the electromagnetic force. Because Maxwell's equations are

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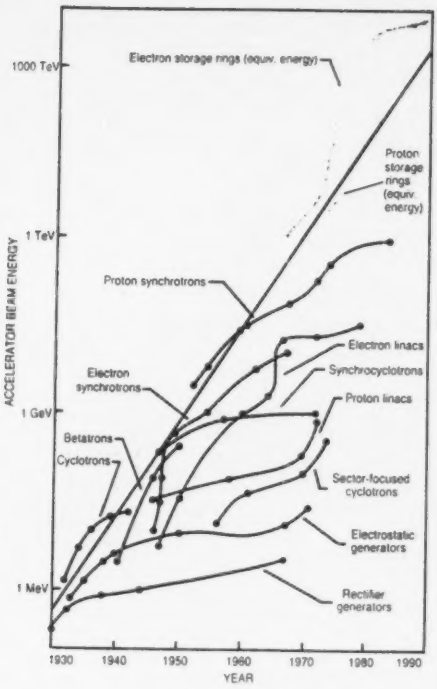


Advance of accelerator beam energies since 1930. Though each successive technology eventually saturates, the envelope (red) of all these "Livingston curves" exhibits an inexorable exponential growth, maintained by the continued introduction of new techniques. Because our desires grow faster than the cost per volt falls, modern accelerators are very much more expensive than their predecessors. (For storage-ring colliders (blue) we plot the equivalent beam energy a fixed-target machine would need for the same center-of-mass energy.) **Figure 1**

linear, it is not difficult to construct a rather general theorem that provides a useful categorization of all accelerators.[7] Specifically, one can show that if a relativistic particle moving in vacuum at constant velocity interacts with any configuration of electromagnetic fields far from all surfaces of dielectrics or conductors, there can be no net acceleration of the particle. In particular, the theorem implies that the very intense electric fields at a laser focus (10^6 MV/m, a hundred thousand times greater than the accelerating field of the SLAC linac) cannot be used to accelerate extremely relativistic particles.

At first sight it seems surprising that one can accelerate relativistic particles at all. But even relativistic particles do not move with strictly constant velocity, and accelerators can be built with conductors near the beam. As it traverses a medium or passes near conductors or dielectrics, an electromagnetic wave slows down, and it can be made to resonate with a traversing particle. In this process, the wave develops a longitudinal accelerating component. Alternatively, we can bend the particles into serpentine trajectories; but this becomes more and more difficult at relativistic energies. A summary of these considerations is presented in the box above, which shows the division of proposed linear accelerators into two categories:

▷ those based on slowing down the electromagnetic accelerating drive

▷ those based on bending the paths of the particles.

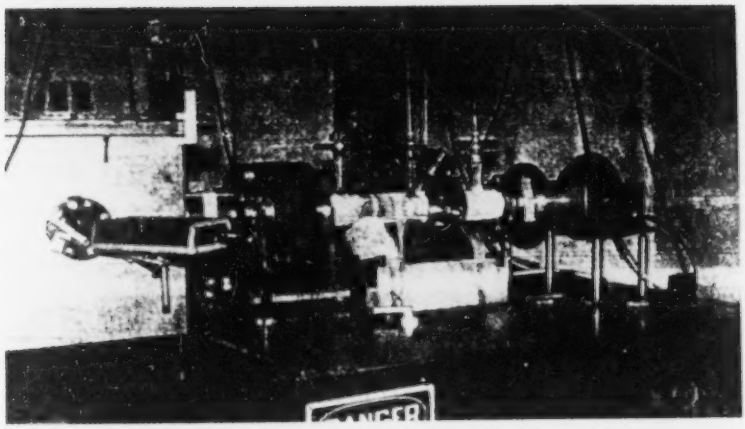
We already have many accelerators based upon conventional slow-wave structures (item 1a). Typically, for electrons, these are disk-and-washer structures—for example the 2-mile-long SLAC linac. New structures (item 1b) are currently being modeled; they will be important at very short accelerating-field wavelengths such as those conveniently obtained from lasers. The exploitation of short wavelengths has also motivated the study of item 1c, dielectric slabs. By passive media (item 1d) we mean polarizable media, such as a gas below breakdown; into this category would fall the inverse Čerenkov-effect accelerator. Plasma accelerators (item 1e) are very different from conventional accelerators, and we shall see that they have great promise (and, needless to say, great problems).

The second general means of achieving particle–field resonance, namely bending the particles, is also the basis for the free-electron laser. (In an FEL, however, the

Acceleration Alternatives

An electromagnetic wave in free space cannot accelerate particles traveling in a straight line. There are two classes of alternatives:

1. Slowing the electromagnetic wave; particles go in a straight line.
   - a. conventional slow-wave structures
   - b. new slow-wave structures such as "picket fences"
   - c. dielectric slabs
   - d. passive media
   - e. plasma media
2. Bending the particles; electromagnetic wave goes as in free space.
   - a. wiggling the particles with a static magnetic field
   - b. wiggling the particles with an electromagnetic wave
   - c. Allowing the particles to undergo cyclotron motion while proceeding longitudinally



Beat wave accelerator experiment at UCLA. Two laser beams and a 1.5 MeV electron pulse enter the plasma chamber from the right. The laser beat frequency excites a longitudinal density wave in the plasma, which the accelerating electrons ride like surfers. The fan-shaped structure houses the diagnostics. Figure 2

particles are being decelerated.) These are "fast-wave devices." That is to say, the walls do not have to be within a wavelength of the particles. Running a free-electron laser backwards would make an accelerator, and such devices have been proposed. However, any accelerator that employs trajectory bending will not be effective at very high energy, because of synchrotron radiation loss. Nevertheless, this limit is rather high—on the order of hundreds of GeV.

I will discuss four novel acceleration schemes, as well as several less radical approaches. The first two novel schemes are active-media devices: a laser-plasma accelerator and a plasma-wake-field accelerator. The other two novel proposals are slow-wave structures: the wake-field accelerator and the pulsed-power linac. In fact, most accelerators are slow-wave structures, and we shall see that within the confines of conventional structures there is still room for considerable variety. I shall consider four such "semiconventional" acceleration schemes.

Laser–plasma accelerator

The laser–plasma accelerator in its first form was the beat-wave accelerator invented by John Dawson and Toshi Tajima at UCLA in 1979.[8] They proposed illuminating a plasma with two intense laser beams of angular frequencies ω_1 and ω_2, whose difference is just the natural plasma oscillation frequency ω_p. In this resonant condition, the plasma becomes very strongly bunched. That is, it develops a longitudinal density wave, which can then be employed for acceleration. The transverse electromagnetic field, quite ineffective for acceleration, has been converted into a longitudinal wave by this process. The accelerating electrons ride the plasma wave much like surfers. This development of "ponderomotive wells" can also be regarded as stimulated Raman forward scattering.

To determine the effectiveness of this process, and thus to appreciate its great promise, let us estimate the accelerating field one might obtain in a plasma. If the plasma bunches completely, then E_L, the longitudinal, accelerating component of the electric field is given by

$$eE_L = mc\omega_p = 4\pi r_0 nc^2$$

where e, m and r_0 are, respectively, the charge, mass and classical radius of the electron, and n is the plasma density. In a plasma, one can obtain densities of 10^{17} cm^{-3}, so that $eE_L = 2\times10^4$ MeV/m. This is a very large gradient indeed!

Now we need to ask to what extent the plasma will bunch. Ponderomotive wave formation can be estimated analytically, and also evaluated numerically by particle simulation. The two approaches agree very well and indicate that bunching is incomplete; the accelerating gradient eE_L is reduced by a factor

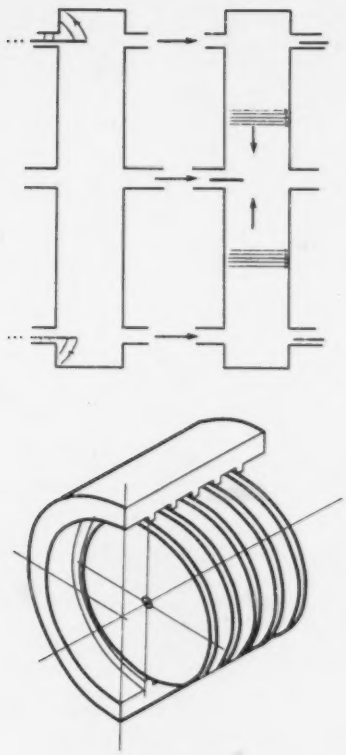
$$\epsilon = \left[\frac{16}{3}\left(\frac{eE_1}{mc\omega_1}\right)\left(\frac{eE_2}{mc\omega_2}\right)\right]^{1/3}$$

where E_1 is the field strength of the input laser at frequency ω_1. In practice, even taking the rise time of the laser into account, ϵ can be as large as 0.7, so that one can expect to achieve very large acceleration fields.

One must make a rather uniform plasma along the length of the accelerator to maintain resonant excitation of the plasma wave. Recently a group at the Rutherford–Appleton Laboratory has obtained adequate uniformity in a "long" plasma 8 mm in length.[9]

Plasma stability is the really important point. To avoid ion effects such as "ponderomotive blowout," one must have a very short laser pulse—shorter than the ion plasma period. Even for such short pulses, adverse plasma phenomena such as laser filamentation and laser self-focusing can occur. In order to study these phenomena, two-dimensional particle simulation studies have been performed, supporting the analytic argument that one can probably find parameters that yield beneficial laser self-focusing but no filamentation.

Study has also been made of how one can add acceleration units in tandem. For one unit alone, the accelerated particles will soon pass the bottom of the



Wake-field transformer accelerating structure. This chain of disk-shaped cavities permits a ring-shaped bunch of low-energy electrons (blue) to drive a following bunch of high-energy electrons (red) along the axis. The driving bunch leaves behind a wake field (yellow), which is scraped off by the structure and transmitted inward toward the axis, where it becomes a strong accelerating field. Figure 3

ponderomotive well and begin giving energy back to the ponderomotive wave as they proceed up the next hill. Thus the accelerator must be terminated at an appropriate length. The maximum energy that particles can obtain in a single unit is given by

$$W_{max} \simeq \left(\frac{2\omega_L^2}{\omega_p^2}\right) mc^2$$

This maximum decreases with increasing plasma density, while the accelerating gradient eE_L increases with increasing plasma density. Thus, operation at a reasonably high acceleration gradient requires the "staging" of several accelerating units.

One must, of course, be concerned with the efficiency of the accelerator. The overall efficiency of the laser–plasma accelerator is a product of component efficiencies: η_L for generating the laser light, η_w for exciting the plasma waves, and η_p for transferring the wave energy to the accelerating particles. The full efficiency, $\eta_L \eta_w \eta_p$, can easily become quite small.

Experiments have been started on the laser–plasma accelerator, or the more modest goal of generating beat waves, at UCLA, Rutherford–Appleton Laboratory in England and INRS-Energie in Canada. Figure 2 shows the experimental setup at UCLA. The UCLA group, led by Chan Joshi, and the Canadian group, led by Francois Martin, have achieved[10] accelerating gradients of the order of 500 MeV/m. The Canadian group has succeeded in accelerating electrons from 0.6 MeV to 2.5 MeV.

The wake-field accelerator

The wake-field accelerator is based on the observation that an intense beam, passing near a conducting surface, will leave fields behind—a wake. This is a well-known effect that has been calculated and measured in great detail. Generally it is regarded as an adverse effect; intense beams passing through a cavity leave behind a wake that "loads down" a cavity and makes it "droop" in energy.

It was pointed out by Gustav Voss and Thomas Weiland at DESY in Hamburg in 1983 that an accelerator could be based upon use of the wake field.[11] It turns out, however, that if the beam to be accelerated is located in the same region of space as the wake-producing beam, the wake will not be particularly large. What is needed is a "wake-field transformer." Figure 3 shows a possible realization of this concept. A ring-shaped beam of low-energy electrons passing through a cylindrical conducting structure leaves behind a wake, which is then transmitted radially inward to become a strong accelerating field on the axis, along which the high-energy beam is traveling. In this way one might obtain accelerating gradients of 150 MeV/m. The primary difficulty is to create and control the wake-producing beam. If it is well defined and short in length, it will produce a strong wake. An experiment to prove the principle of this new mechanism is now under way at DESY. First results, in which electrons have indeed been accelerated, demonstrate the validity of this concept.

The plasma-wake-field accelerator

Pisin Chen and his colleagues at UCLA pointed out in 1985 that an efficient way of producing the plasma wave for a laser–plasma accelerator would be to make use of a particle beam.[12] One would excite the plasma with an intense bunch train of high-energy electrons rather than by the beat wave of two laser beams. Because intense bunch trains are readily available, it is relatively easy to study this mechanism experimentally, and such work is currently under way in a collaboration between the University of Wisconsin and the Argonne National Laboratory.[13]

The plasma-wake-field accelerator is subject to the same restriction I've discussed above for wake-field accelerators in general. Because none of the plasma-wake-field configurations proposed so far are transformers, the plan is to tailor the driving pulse train so as to make the wake significant and thus achieve strong excitation of the desired plasma wave.

The switched-power linac

The switched-power linac is a slow-wave structure conceptually quite similar to the wake-field accelerator. Both employ a cylindrical geometry and achieve enhancement of the accelerating voltage pulse by means of a radial transmission-line pulse transformer. The difference is that in the wake-field transformer the pulse is created by an intense electron ring, whereas in the switched-power

linac a current pulse would be produced by switching—probably by a laser pulse on a photocathode. The concept has been conceived and developed by William Willis and his coworkers at CERN and Brookhaven.[14]

The scheme exploits two ideas in particular: that a distributed power source leads to high power density and thus high accelerating gradients; and that a short pulse length implies high peak power, low stored energy and thus limited heating. The high power density requires a high, pulsed charging voltage. High gain requires that the switched energy be very much larger than the laser energy, and high efficiency requires that the switched energy be comparable to the stored energy. Thus the switched-power concept quickly leads to a study of high-voltage breakdown, to the development of a photocathode and its associated laser, and to the problem of aligning the laser with the photocathodes and the disks of the accelerator structure. A scale model, built at CERN, is shown in figure 4. The development program there has been going on for two years. At least another two years will be required to demonstrate one stage of a switched-power linac.

An alternative to the photocathode switching under consideration at CERN and Brookhaven would be a solid-state switching scheme being studied at the Universities of Rochester and Osaka. There are questions of efficiency, jitter, high-voltage breakdown, lifetime, repetition rates and the like. In particular, the photocathodes would have to operate in the novel situation in which the photon flux is very high and brief and the electric fields on the switch are very strong. Initial studies have achieved photocurrent densities of 10^4 A/cm^2 with surface fields of 0.06 GV/m (gold-tungsten wire cathodes of 50-μm diameter). Work is now devoted to increasing the field to about 1 GV/m, to obtain 10^6 A/cm^2 with a quantum yield of 10^{-2}. This would represent an order of magnitude increase over present quantum yields.

In the switched-power scheme, as in the wake-field and laser-plasma accelerators, one faces the important question of reproducibility. In linear colliders, one imagines bringing into collision beams that are a fraction of a micron wide. Such extreme beam compression is necessary if one is to obtain useful collision rates with beams that collide only once before they are disposed of. Can these acceleration concepts lead to beams whose jitter from pulse to pulse is much less than a micron? We don't know yet. Conservative people will go with a "conventional linac," where a great deal is known about deflecting modes and jitter. Such people elect to put their inventive talents into developing novel power sources. In the remainder of this article, I will describe some of their efforts.

Two-beam accelerators

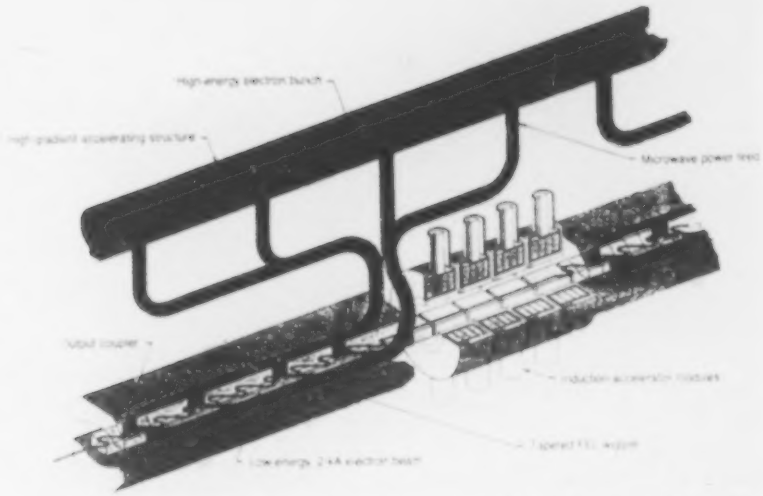
Two-beam accelerators have been studied for some years now by our group at the Lawrence Berkeley and Lawrence Livermore Labs and, more recently, by Wolfgang Schnell and his coworkers at CERN.[15] The concept is illustrated in figure 5. Instead of many thousands of power tubes arrayed along the length of the accelerator, one would employ a single high-intensity driving beam of low-energy electrons, parallel to the high-energy beam one seeks to accelerate. The idea makes sense because it is rather easy to make high-power beams of low-energy electrons. This same capability is also an important reason for the great interest nowadays in high-power free-electron lasers.

The two-beam scheme is essentially an attempt to miniaturize conventional rf accelerating structures so that one obtains extraordinarily large accelerating gradients with high efficiency. The low-energy driving beam provides the short-wavelength rf power essential to this miniaturization. We are speaking of wavelengths on the order of 1 cm, in place of the 10-cm microwave power that present-day rf linacs get from klystron arrays.

The driving beam can give up its energy in at least two ways: It can either be passed through an undulator, thus functioning as a free-electron laser, or it can be bunched and passed through resonant "transfer cavities." As it feeds out microwave power along the length of the accelerator, the driving beam's energy can be replenished by pulsed induction-linac modules or superconducting rf cavities placed along its path. Figure 5 shows a two-beam accelerator with FEL undulators and induction-linac units. An induction accelerator is well matched to the requirements of a collider; it produces high peak power for a short pulse length, and it can be pulsed repeatedly.

The generation of rf power in the two-beam accelerator is very efficient over a wide range of frequencies because energy not given up by the driving beam need not be resupplied by the energy-supply system. Thus, the operating efficiency of a two-beam accelerator can be very high—approaching 100%.

The two-beam concept offers considerable latitude in the choice of rf frequency; it can thus be chosen for linear-collider considerations rather than by consideration of the availability of power sources. As the rf frequency f is



Two-beam accelerator envisioned by the Berkeley-Livermore group. A high-gradient miniature of a conventional rf accelerating structure is fed short-wavelength microwave power from an adjacent high-current beam of low-energy electrons serving as a free-electron laser as it undulates through wiggler magnet arrays. The energy this driving beam gives up to the high-energy electron beam is replenished by induction-linac modules along the way. **Figure 5**

though it. The efficiency of this conversion was greater than 40% from beam energy to microwave energy. Problem areas that remain to be studied, and there are many, include phase control of the rf, transverse stability of the driving beam, and the transport of intense microwave power from the FEL to the acceleration gaps.

All of these problems appear to be solvable, but at the expense of simplicity. The phase control of the rf, for example, is particularly difficult. Because of the powerful amplifier action of the FEL, the rf phase is sensitive to beam current, wiggler field and beam energy. So far, the only method devised to control phase is to discard the rf when its phase is unacceptable and reinsert proper-phase rf at a small cost of emittance growth in the driving beam wiggling its way through the FEL.

The transfer-cavity alternative, often called the relativistic klystron version of the two-beam accelerator, shown in figure 7, is now the object of considerable effort by a SLAC–Berkeley–Livermore collaboration, and also by the CERN group. It does seem to remove some of the problems associated with the FEL, but one should note that the FEL has been demonstrated to produce power, while the relativistic klystron thus far exists only on paper. People have investigated transfer-cavity problems such as longitudinal and transverse beam dynamics and the proper design of the cavities. Transverse beam dynamics is of particular concern because the very small transfer cavities enhance transverse instability, causing "beam breakup."

Klystrons, laserrtrons and gyroklystrons

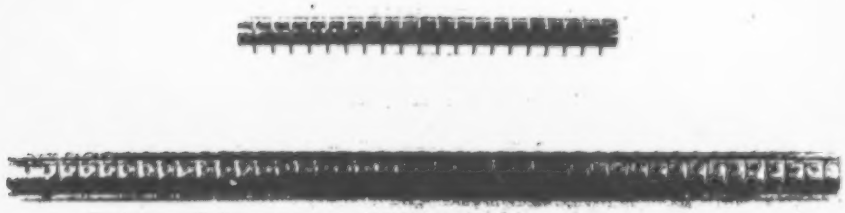
Finally, I will describe three other possible sources of short-wavelength power for a high-gradient rf linear accelerator.

The klystron, until now the power amplifier of choice for electron–positron linear colliders, is an electron-beam tube usually containing two cavities. In the first cavity a velocity modulation is imposed on the tube's electron beam and the bunched beam then radiates coherent microwaves into the second, tuned output cavity. The SLAC linac operates in the "S band," at a wavelength of 10 cm, so that S-band klystrons have been the subject of extensive development. Having been upgraded to serve as the accelerator for the Stanford Linear Collider, the linac now accelerates electrons and positrons to 50 GeV. More than two hundred 65-MW klystrons line the great gallery atop the linac, feeding it rf power in 3.5-microsecond pulses.

A klystron operating in the S band has produced 150-MW peak power in a pulse of 1 microsecond. The high-energy linear colliders we envision for the next generation will have to operate at significantly shorter wavelengths. Experiments at SLAC have shown that an accelerating gradient of more than 200 MeV/m can be achieved at the 3-cm wavelength regime, the so-called X band. As klystrons are designed for higher frequency, the output power decreases as the cavities must be made smaller, reducing the klystron's beam current.

A development program is under way at SLAC to build a 100-MW, 2-μsec klystron operating in the X band with 65% efficiency.[17] The rf pulse will have to be further compressed because at these wavelengths the rf must fill the accelerating structure in 100 nanoseconds. A compression scheme can be devised, but it requires very long sections of waveguide (2000 feet for a 2-μsec pulse). For still shorter rf wavelengths, conventional klystrons do not seem to be suitable for linear colliders.

A second development program at SLAC, in collabora-



High-gradient accelerator section, 10 cm long, fabricated at Livermore as part of the two-beam accelerator study. The completed copper disk-and-washer structure (bottom) is made by electroforming copper over an aluminum "mandrel" (top) and then removing the aluminum with strong hydroxide. This miniature rf structure is resonant at 1 cm, an order of magnitude shorter than the wavelengths of present-day high-energy linacs. Figure 6

tion with Livermore, employs pulse-forming devices, using saturable inductors pioneered at Livermore to make a short-pulse (50–100 nsec) klystron.[17] They are planning to put a megavolt pulse on an X-band klystron in the expectation that a peak power on the order of a gigawatt may be obtained from klystrons at very short pulse widths. The development confronts problems associated with large cathode–anode stresses and with the cathode current-density responses to the fast rise times (15 nsec) of very short pulses. The group aspires to efficiencies on the order of 70%.

The lasertron is a microwave power tube that employs a photoemission cathode illuminated by an optical pulse train of the proper microwave structure to produce a bunched electron beam directly at the cathode. This lasertron beam is accelerated to high voltage and immediately passed through an rf output structure to produce microwave power for the linac. In a high-peak-power device there is considerable charge in each bunch, requiring that the lasertron beam be confined transversely by an axial magnetic field, and that it be rapidly accelerated to limit longitudinal spreading. As in all microwave power tubes, the spent lasertron beam is dumped in a collector. Lasertrons are under active development at SLAC, KEK and Orsay.[18] The idea is also being studied at the Texas Accelerator Center and at Los Alamos.

The attractiveness of a photoemission cathode stems from the ease with which one can directly modulate the emission at high frequency, and from the very high current densities one can readily obtain. The lasertron development program at SLAC involves learning to prepare robust high-current-density photocathodes capable of operating for long periods in the environment of a high-power microwave tube. Because photoemission cathodes are typically far more sensitive than thermionic cathodes to degradation or destruction by residual gases, one needs a very good vacuum system. Furthermore, photocathodes generally require alkali metals for their activity, which makes the operation of high-voltage guns more difficult.

The laser itself is a complex system, far more costly than a filament transformer. The necessity of getting the illuminating light to the cathode can add significantly to the complexity of the basic device. In general, photoemission cathodes operating in actively pumped ultrahigh-vacuum systems have not shown very long lifetimes, and the ability of these cathodes to deliver the large total charge required over the operating life of a microwave power source has not yet been demonstrated.

On the other hand, the lasertron is in many ways simpler than thermionic devices, so it may be possible to produce a relatively inexpensive unit. Because the photocathode emits only while illuminated, only a dc power supply or a very simple modulator is needed, as opposed to the complex and expensive modulators necessary for devices with thermionic emission cathodes. This is true, however, only if the very large voltages required to accelerate bunches before they disassemble due to space charge can be held in dc.

If they are to be useful for future linear colliders, lasertrons must be extended to shorter wavelengths. The lasertron is, however, subject to the same wavelength scaling law as the klystron; both give decreased output power as the frequency is increased. Recently, the Texas group has proposed a non-axial-symmetric "sheet beam" lasertron for delivering high power at high frequency.

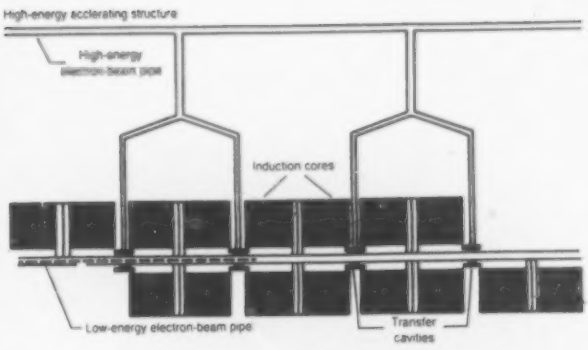
Gyrotron oscillators have been developed for heating thermonuclear fusion plasmas at short wavelengths (5 mm), with very high average power (300 kW), high peak power (300 MW) and high efficiency (50–65%). Although some effort has been made to develop gyrotron amplifiers (as distinguished from oscillators), a group at the University of Maryland has only recently begun to develop a high-peak-power gyrotron tube.[19]

The basic idea of the gyrotron (also called a cyclotron-maser or gyroklystron) is to produce a near resonance between a harmonic of the electron cyclotron frequency and an output cavity mode. A longitudinal magnetic field causes the gyrotron's spiraling electron beam to bunch, thus generating coherent short-wavelength radiation.

The suppression of unwanted modes is the paramount issue here. Also of concern are wall losses and space-charge depression, both of which must be kept within acceptable bounds. These considerations work in opposite directions; the suppression of unwanted modes is most easily achieved by using a small cavity in which few modes are excited. On the other hand, wall losses are minimized by working in a large cavity, and space-charge depression is minimized by using a large-radius, low-density beam.

Phase stability in the gyroklystron is a crucial consideration for use in large linear accelerators, where hundreds or even thousands of microwave amplifiers must be synchronized. While this is a demanding requirement, especially for the modulator, it is within the state of the art. Because prior studies of gyroklystrons have been plagued by spurious oscillations, stability is of special concern.

The Maryland design is for a 10-GHz tube producing 2-μsec pulses with a peak power in excess of 30 MW and an efficiency of 45%. Studies indicate that the design can be scaled up to power levels exceeding 150 MW. This would



Relativistic klystron version of the two-beam accelerator. Instead of passing through an FEL wiggler, the driving beam of low-energy electrons is bunched and sent through resonant transfer cavities, thus producing copious microwave radiation, which is sent to the high-gradient accelerating structure. Here we show one period of such an accelerator. Figure 7

be accomplished by increasing the voltage, radius and current of the tube's beam while maintaining its intensity. Just as in the conventional klystron, the gyroklystron's output pulse would have to be further compressed so that it matches the requirements of the accelerating structure.

Continuing the growth

There is clearly no lack of ideas for new acceleration schemes. They range from evolutionary developments of conventional power sources to completely new concepts such as the laser-plasma accelerator. Furthermore, there are already modest experimental programs associated with each of the approaches discussed here. Success in some of these developments is crucial to the building of future high-energy electron–positron colliders.

But conventional accelerators are very effective and very large. Something really new is not going to compete until it has undergone extensive development and modeling. That process will be lengthy and expensive, but necessary if we are to continue to see, as we have in the past, the exponential rise in the accelerator energy available to high-energy physicists shown in figure 1, and hence the continuation of high-energy physics as an experimental science.

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Senator FORD. What I would like to do is now ask you three gentlemen to retire for just a few moments, and we will ask the other two scientists, Dr. Quigg and, I am not sure everybody likes to pronounce their name so, Stanley, will you pronounce your last name?

Dr. WOJCICKI. Wojcicki.

Senator FORD. Wojcicki, all right sir. It was pronounced correctly when you pronounced it. I want you to know that.

Dr. WOJCICKI. I hope so. [Laughter.]

If I can do nothing else, I hope I can do that.

Senator FORD. So we will just let you start, Stanley. We got on a first name basis here right quick.

STATEMENT OF DR. STANLEY WOJCICKI, DEPUTY DIRECTOR, EXTERNAL RELATIONS, SSC CENTRAL DESIGN GROUP

Dr. WOJCICKI. Mr. Chairman, Senator Domenici, I would like to thank you for this opportunity to allow me to express my views on the issues of science funding in this country.

According to your directions beforehand, I will try to be very, very brief, and only extract several of the points from my previously prepared testimony.

Few of us will question that the high standard of living we are enjoying today is due to past discoveries of fundamental laws of nature and their subsequent application to everyday life.

Similarly, the quality of our life, and our relative standing among nations in the 21st century, will depend very much on the investments we are willing to make today in basic research.

I fear, however, that in spite of the significant recent advances, the level of basic research funding in this country is still insufficient. I would like to elaborate on some of my reasons for this belief.

Today, about 12 percent of our research and development funds are spent on basic research. I believe that this is too small when one considers that all of the subsequent applied research, development, and manufacturing are founded on basic research.

Alternatively, we can compare our expenditures in this area with those of our major trading partners. Our number of 12 percent that I quoted a moment ago can be compared with 22 percent of the research and development budget that goes to basic research in West Germany, and 13 percent in Japan.

However, what is probably even more significant is the fact that the rate of increase in those expenditures for basic research is growing significantly faster abroad than it is in this country.

Senator DOMENICI. Doctor, is that 12 percent the national or the Government's?

Dr. WOJCICKI. That is national.

Senator DOMENICI. So you have left out all the research being funded at State universities by——

Dr. WOJCICKI. No, I am sorry. I meant to say that is the national, that is not only the Federal government, that is the total support.

Senator DOMENICI. Everybody.

Dr. WOJCICKI. That is correct, yes. In light of those facts, I would like to argue that we as a Nation must try to increase our support

of excellent basic research across the broad spectrum of the frontiers of knowledge.

And this brings me to my second point, namely, what are the criteria that one could use to define excellence in basic research? I would like to suggest five of those criteria, and then try to show how the project we are discussing today, the SSC, stacks up against those criteria.

Very briefly they are, the intellectual excitement of the project, namely the extent to which it addresses the key problems in the field; the support within the subfield for the project; its essential nature for the U.S. preeminence in the field; its technical feasibility and readiness; and finally, the fiscal soundness and the feasibility.

I believe it is true that even the opponents of the SSC will give it high marks for the first four criteria.

What are the fundamental constituents of matter? What are the forces that govern them? How did our universe begin? Where does the mass come from? All of these are basic questions on which the SSC is bound to make profound impact.

As far as the second criterion is concerned, I should mention that the SSC has been proposed as the essential project required for the continuing vitality of the U.S. high energy physics program after an extensive six-month deliberation and consideration of many other options.

Since that time, this need has been reaffirmed on a number of occasions. And many key people in the field, both here and abroad, have been continuing to contribute to its physics and technology issues.

Regarding the third criterion, I would like to point out that new initiatives around the world are rapidly relegating the U.S. to the role of bystander in this field. There is a new high energy electron-positron collider facility being built at CERN, a new electron-proton collider in West Germany, and a proton-proton collider in the Soviet Union.

With all those new frontier research opportunities abroad, unless the U.S. goes ahead with the SSC, our high energy physics program will soon be eclipsed by those other initiatives abroad.

The technical feasibility and readiness of the supercollider has been fully documented in the conceptual design report. That is a 700-page report, together with some 2,000 additional attachments, that was the result of the R&D effort on SSC, sponsored by the Department of Energy during the last several years.

This document has been extensively reviewed by a number of different review committees. And the results of those reviews became the basis of the decision by the Executive branch to go ahead with the project.

So finally that brings me to the last criteria, namely the fiscal feasibility and the fiscal issues regarding the SSC. This is undoubtedly the most controversial issue, but I think one that should be kept in proper perspective.

It is certainly true that SSC is a very expensive scientific instrument; $4 billion is a lot of money by standards of most people. But this figure, however, should be kept in proper perspective.

First of all, we have to remember that it has to be integrated over a period of eight to 10 years of construction, and thus cannot be compared with the annual expenditures in many other fields.

Secondly, it represents a dramatic reduction, thanks to technological improvements, in the ratio of cost per attainable energy. Even with respect to the Fermi LEP Tevatron, recently completed, it represents a factor of about seven improvement in cost per energy.

Finally, we should put the SSC cost in the proper historical perspective. If you take the profile requested by the Administration, even at the projected peak of its construction expenditures, the total high energy physics expenditures in this country, if expressed as a fraction of the GNP, will be no higher than it is today in Western Europe, and it will only begin to approach the levels that we had in the 1960s during the time of construction of the Fermilab.

I would like to conclude with just a brief remark. For over 2,000 years, mankind has tried to understand the basic composition of matter, the nature of basic forces, and the origin of the universe.

Thanks to recent advances, we think that today we are very close to the threshold of that understanding. It would really be tragic if we, the richest nation ever, were to shirk away from what is not only a great challenge but also a great opportunity.

Mr. Chairman, Senator Domenici, thank you very much.

[The prepared statement of Dr. Wojcicki follows:]

Statement of Stanley Wojcicki
SSC Central Design Group
Universities Research Association

before
The Senate Subcommittee on Energy Research and Development

April 12, 1988

Mister Chairman and Members of the Subcommittee:

I want to thank you for this opportunity to offer my views on the funding priorities for basic science facilities of the Department of Energy. It would be presumptuous and not very productive for me to attempt to rank various scientific initiatives proposed by the Executive Branch. I have not studied the full program in sufficient depth to make a meaningful comparison possible, and any list of priorities would undoubtedly be flavored by personal interests and prejudices. I believe that such a prioritization across a wide spectrum of disciplines is impossible to achieve in an objective way. The identical conclusion was drawn by a recent Physics Survey Panel, sponsored by the National Academy of Sciences and chaired by Dr. William Brinkman. After more than 100 distinguished physicists had wrestled with this problem for over a year, they limited themselves to an assessment of progress, needs, and prospects in various subdisciplines of physics.

I thought it would be more useful to this committee if I were to limit my remarks to two general areas: the reasons for vigorous federal support of basic research and the criteria that one should use in deciding whether a given scientific initiative is worthy of support.

Few of us will question that the high quality of life we enjoy today is, to a very large measure, the result of past investments in basic research. Our electrical technology, the foundation of modern industrial society, is the result of curiosity-driven investigations into the nature of electricity and magnetism by Faraday, Maxwell, Hertz, and other pioneers in this field. Attempts to understand the emission spectra of different elements led to the establishment of quantum mechanics and hence to invention of the laser and transistor. Study of the nucleus gave rise to today's nuclear weapons, nuclear power, and nuclear medicine. Investigations in molecular biology gave rise to endless possibilities in

genetic engineering. The examples go on and on. We can be certain that our future depends just as strongly on the investment that we are willing to make in basic research today.

It is the very nature of basic research that its fruits are unknown a priori. Furthermore, basic research has many inherent uncertainties; the area where it will make the most impact cannot be predicted accurately beforehand. For these reasons the Federal Government is the most appropriate sponsor of basic research activities.

Although our policy makers appreciate the need for strong federal sponsorship of basic research and have increased support over the last few years, I have great concern that the level of support is still grossly inadequate. Let me mention some figures. We have fallen behind our major economic competitors, Japan and West Germany, in terms of the fraction of GNP that we devote to basic research. A recent article in Fortune magazine, which acknowledges the strong R&D base for science and technology that the U.S. has built up, contains some disquieting statistics. I quote:

> Despite constraints on federal spending, support for basic research in the U.S. is on the upswing. Over the past decade, government appropriations rose 60% in constant dollars. Contributions from industry for the same period grew even faster, by nearly 70%, following nine years in which industrial spending for basic research fell at an average annual rate of 2.3%.
>
> But in the same period, total German spending doubled and Japanese outlays tripled. Japan, not previously renowned for basic research, now devotes to it 13% of total R&D spending, a slightly greater proportion than the U.S., where 12.2% goes for that purpose. . . .
>
> West Germany epitomizes the new European commitment to basic science. As a percentage of GNP, the West Germans spend as much on total R&D as the U.S. But an impressive 22% of their R&D budget goes to basic research . . . .

The relative fraction of total R&D budget that goes to basic research in this country, 12.2%, is, in my opinion, disproportionately low. All applied research and all development builds on and utilizes the fruits of basic research. By devoting only one-eighth of the R&D budget to basic research, we are missing irreplaceable opportunities.

In absolute terms, the 1988 Federal expenditure for basic research is about $9B, less than 1% of the total Federal budget. (This amount is $2B less than was spent last year in the U.S. for cosmetics.) The whole nation, including the Federal government, state governments, both private and public universities, private foundations, and industry, spends but $15B on basic research, about 1/3% of our total gross national product. In light of these numbers, I cannot accept the often-stated claim that we cannot afford adequate support of basic research. On the contrary, I would argue that we as a nation need to pursue excellent basic research all across the frontiers of knowledge. This brings me to my second major point -- my views as to the criteria that we should apply in judging major new scientific initiatives.

I would like to suggest five specific criteria and show briefly how the project that I am involved in, the Superconducting Super Collider (SSC), stacks up when evaluated in terms of those criteria. My colleague, Dr. Quigg, will elaborate in more detail on some of those issues in his testimony.

1) **Intellectual excitement of the project** (i.e., the ability to address the key problems in the field). There appears to be very little controversy on this point, even by those who question whether we should proceed with the SSC. During the course of this century we have seen remarkable progress in understanding the nature of the fundamental constituents and forces that govern our universe. But at the same time we have come to the realization that some key questions can only be answered by new and more powerful instruments. The parameters of the SSC were chosen so that it will be able conclusively to address these questions. The continuing elucidation of the underlying order of the universe is one of humankind's highest achievements. To read in a history book of the future that America had abdicated this pursuit, especially after so many successes, would be extraordinarily sad.

2) **Support within subfield**. The need for an SSC-like machine was first formally enunciated by the U.S. high-energy community almost five years ago. This decision was endorsed unanimously by a High-Energy Physics Advisory Panel (HEPAP) subpanel after six months of extensive deliberations and then forwarded to DOE as a unanimous recommendation of the parent HEPAP. (I might add that the subpanel included among its members the former Director General of CERN, Sir John Adams, and the present CERN Director Designate, Professor Carlo Rubbia.) The best people in the field have been

contributing to the continuing study of the machine, the detectors required, and to the physics to be explored.

3) **Essential nature for U.S. preeminence in the field.** The field of high-energy physics, by the very nature of the problems it deals with, depends on advanced technology. High-energy accelerator-collider complexes are key components of this technology. It will be impossible to maintain preeminence in the field without investment in these facilities.

Today three kinds of colliders are believed to be essential to explore the different aspects of subnuclear physics. Electron-positron (e^-e^+) colliders are ideally suited to address one category of problems. The most energetic of these machines will be LEP, which is being built at CERN and is scheduled to achieve its full energy of 200 GeV around 1993. An electron-proton (e^-p) collider, the first of its kind, is being built in Hamburg, West Germany, and will continue the study, at a much higher energy, of the spectrum of physics issues first investigated very successfully at the Stanford Linear Accelerator Center (SLAC). No such machine is being contemplated in this country. Finally, the third class of colliders are hadron-hadron colliders, either proton-proton or proton-antiproton (pp or $p\bar{p}$). At the present time, the most powerful of these is the Tevatron at Fermilab, with energy of 1.8 TeV. In several years, however, that machine will be eclipsed in energy by the Soviet machine with a planned energy of 6 TeV. The Europeans are discussing installation of a hadron collider (LHC) in the LEP tunnel with a potential of achieving 15 TeV total energy. The SSC would reestablish the U.S. lead in this area with a proton-proton collider of 40 TeV. (I might add that another kind of facility that is complementary to the colliders is a large underground laboratory that allows one to search for proton decay, cosmic sources of neutrinos, monopoles, etc. Here, too, the Europeans have taken the lead with the recently completed Gran Sasso facility in Italy.)

Parenthetically, I might point out that the above discussion illustrates the worldwide nature of high-energy physics and the kind of international collaboration that is going on in the field. Several kinds of complementary facilities are required to fully explore the subnuclear world, just as several kinds of different telescopes (optical, radio, x-ray, gravitational) are needed to properly investigate the far reaches of our universe. Rather than duplicating these facilities in several

different regions of the world, they are open to all scientists on the basis of scientific merit of the proposal and demonstrated competence to carry them out. For the system to work, however, each region must contribute its share. The SSC will be the U.S. contribution to this common pool.

4) **Technical feasibility and readiness.** Over the last four and a half years the DOE has supported R&D and design of the SSC. That work has produced a Conceptual Design Report, a comprehensive volume of some 700 pages with another 2000 pages of attachments. This document has been extensively reviewed by a number of groups and found to be technically and fiscally sound. It formed the basis of the Presidential decision to approve the project. Subsequent R&D work, which has further developed various technical details, resulted in our readiness today to involve industry in the manufacture of pre-production models of the main technical components. The successful operation of the Fermilab Tevatron—the world's first superconducting accelerator—and the construction of the superconducting proton accelerator in West Germany indicate that superconducting accelerator magnet technology has come of age. Furthermore, it is clear that no other technology can achieve the scientific parameters of the SSC. Recently, a number of distinguished panels have been convened, both by U.S. government agencies and by the National Academies, to examine some of these technical issues. Invariably they have concluded that other technologies are at least 10-20 years away from being able to offer alternatives to the SSC.

5) **Fiscal soundness and feasibility.** This is an apt issue, because the SSC is indeed an expensive instrument. However, its cost has to be kept in the proper perspective. First of all, the SSC construction cost will be spread out over almost a decade and thus cannot be compared with annual expenditures in other programs, as is often done. Second, extensive fiscal reviews and the excellent record of previous construction projects in the field provide a reliable basis for believing that the SSC cost estimates are sound and dependable. Third, thanks to technological improvements, the SSC represents a dramatic reduction in the ratio of cost to energy. Compared with the Tevatron, it represents a factor of seven improvement when both costs are expressed in the same year dollars. Finally, we must not forget that even at the peak of the SSC construction, total funding of high-energy physics in this country as a fraction of GNP will be no more than what it is in Western Europe today and will approach the level of

the late 1960s, when SLAC and Fermilab were under construction. (I might point out that that period was also one of fiscal constraints, with a major share of our revenues going towards the war in Vietnam and the large technological initiative of putting a man on the moon.)

I have tried to give my views about the criteria that one should use in deciding whether a given scientific initiative should be supported. I have also tried to indicate that the SSC, when judged according to these criteria, merits strong federal support. Before concluding my testimony I would like to express my opinion about some criteria that are not appropriate in judging the value of science projects. Specifically, I would like to address three separate points.

1) Big science versus little science. Comments sometimes have been made that imply that we have to guard against "big science" taking too large a share of the science budget. I want to argue that this is an artificial and inappropriate criterion to apply to science funding policy. Neither big nor small science is worthy of support merely because of its size. Undoubtedly, there are many excellent small science projects worthy of support, just as there are many excellent big science projects. Similarly, there are both big and small science projects that are less deserving of support. Thus it would be meaningless to allocate resources on the basis of size alone.

Exploration of the laws of nature increasingly requires big, expensive facilities. We find examples not only in high-energy physics, but also in astronomy, nuclear physics, condensed matter physics, chemistry, plasma physics, oceanography, etc. I believe that soon it will be difficult to find a discipline of science without some frontier problems that can be addressed only by reliance on big, multi-user facilities.

Big science should be encouraged if, and only if, it will result in better and more efficient use of human and financial resources in attacking cutting-edge scientific problems. From personal experience, I can attest that university high-energy physicists only reluctantly abandoned their small institutional accelerators for the sake of even larger regional, national, or world-wide facilities. However, the frontier scientific problems could be attacked only in that manner, and we have been rewarded with brilliant successes.

2) University infrastructure versus national laboratories (or regional facilities). There is no doubt that university technical infrastructure has been sorely neglected and has witnessed significant deterioration over recent years. I believe that this is only another manifestation of the lack of sufficient support of basic research. Furthermore, I must stress that we cannot view national laboratories as a totally distinct entity from our universities. The national laboratories are a part of the university infrastructure. They are generally run by universities, either singly or in consortia; they provide research opportunities that university faculty and graduate students would never find in even the most well-equipped university.

What does it take to perform an experiment at the frontier of knowledge? In high-energy physics, one needs frontier accelerator facilities that can penetrate the unknown, the capability to design and build the most sophisticated detectors, and sufficient computer power to analyze and interpret the data. It may be possible to have some of this infrastructure on campus; frequently, however, it is not. Accordingly, universities have banded together to build the required facilities on a joint basis. The important point is that one facet is no less important than the other. Without all the components, university research in this particular discipline would deteriorate or even cease completely.

3) Practical utility of basic research. I have already argued that, by the very nature of basic research, the extent of its practical applications cannot easily be identified a priori. Thus, it is unrealistic and counterproductive to set up predictable practical utility as a criterion of support.

However, many benefits are provided to society by the pursuit of basic research, independent of the eventual results. The needs of basic research stimulate development of better technology and techniques in such areas as electronics, cryogenics, machinery, and computing. These developments often have important applications in other applied or basic research, as well as in products of everyday life. Less frequently discussed is the benefit of basic research for young people. Not only does challenging basic research stimulate their interest in pursuing careers in science, technology, and mathematics, it also provides crucial training in experimental and analytical techniques. For these reasons, basic research is the "seed corn" for future commercial innovation.

I would like to cite some concrete examples of the fact that utility of basic research cannot be predicted. Two years ago, superconductivity was not the household word it is today. To mention another example, Lord Rutherford, perhaps the foremost nuclear physicist, expressed great skepticism that anything useful would come out of the study of the nucleus, just six years before the discovery of fission and the momentous change that that discovery brought about. Finally, one of the more eloquent cases for basic research has been made by a director of an industrial research laboratory, H. B. G. Casimir, and I would like to end my testimony by quoting him:

> I have heard statements that the role of (basic) research in innovation is slight. That is about the most blatant piece of nonsense it has been my fortune to stumble upon.
>
> Certainly, one might speculate idly whether transistors might have been discovered by people who had not been trained in and had not contributed to wave mechanics and the theory of electrons in solids. It so happened that inventors of transistors were versed in and contributed to the quantum theory of solids.
>
> One might ask whether basic circuits in computers might have been found by people who wanted to build computers. As it happens, they were discovered in the thirties by physicists dealing with the counting of nuclear particles because they were interested in nuclear physics.
>
> One might ask whether there would be nuclear power because people wanted new power sources or whether the urge to have new power would have led to the discovery of the nucleus. Perhaps -- only it didn't happen that way, and there were the Curies and Rutherford and Fermi and a few others.
>
> One might ask whether an electronic industry could exist without the previous discovery of electrons by people like Thomson and H. A. Lorentz. Again, it didn't happen that way.
>
> One might ask even whether induction coils in motor cars might have been made by enterprises which wanted to make motor transport and whether then they would have stumbled on the laws of induction. But the laws of induction had been found by Faraday many decades before that.
>
> Or whether, in an urge to provide better communication, one might have found electromagnetic waves. They weren't found that way. They were found by Hertz, who emphasized the beauty of physics and who based his work on the theoretical

considerations of Maxwell. I think there is hardly any example of twentieth century innovation which is not indebted in this way to basic scientific thought.

Mr. Chairman, members of the Subcommittee, this concludes my prepared testimony. I shall be happy to try to answer any questions that you may have.

Senator FORD. Thank you very much, Doctor. We will now hear Dr. Quigg.

And Doctor, the last three, the last two to testify are from California, Berkeley, and you are too. And I noticed both of them have been out in the weather more than you have. Or do you just not tan very easily?

Dr. QUIGG. Mr. Chairman, I bear the marks of having had a red-haired father.

Senator FORD. Oh, well, I understand that. There is always an explanation, usually scientific. [Laughter.]

Thank you.

STATEMENT OF DR. CHRIS QUIGG, DEPUTY DIRECTOR, OPERATIONS DIVISION, SSC CENTRAL DESIGN GROUP

Dr. QUIGG. Thank you very much, Mr. Chairman, and Senator Domenici.

We physicists have just celebrated the 300th anniversary of Isaac Newton's discovery of the law of gravity, a monument to human understanding of the physical world.

Not only did Newton explain the elliptical orbits of the planets, but he also showed that the same force of gravity was responsible for the motions of the satellites of Jupiter and Saturn, the movement of the moon about the earth, the tides in the sea, and the fall of an apple from a tree.

In other words, he recognized the universality of the law of gravity. That realization was perhaps Newton's most important and lasting contribution. It is an indication that the same laws of nature hold at all times and in all places. And this is the basis for our conception of the universe as an orderly rather than capricious place, a universe that we may hope to comprehend.

The idea that in laboratories on earth we can learn the physical laws that govern the structure of the universe has had a profound effect on our civilization.

Elementary particle physics, the field that the Super Collider is designed to illuminate is the science of the ultimate constituents of matter and the interactions among them.

It seeks to answer two very simple questions. First, what is the world made of? And second, how does the world work?

In common with other scientists, we hope that by exploring the basic laws of nature, by finding and codifying laws that have a wide scope of applicability, we will better understand our universe and eventually have the possibility of putting that better understanding to humane and productive use.

Over the last two decades, the age-old struggle to describe and comprehend the nature of matter has been rewarded by a radically new and simple picture of nature. This progress has in large measure been stimulated by experimental results from the particle accelerators at Brookhaven, Cornell, Fermilab, and Stanford in the United States, as well as from other machines abroad.

We have learned that all of matter in its diverse forms is assembled from a few basic building blocks called quarks and leptons. All the known natural phenomena can be described in terms of a few fundamental forces acting among these basic constituents.

These insights not only provide a framework for describing and understanding the world around us, but also elucidate the first instance after the creation of the universe.

However, as our conception of matter has become better established, we become increasingly aware of its shortcomings. Since the late 1970s, the international scientific community has recognized that we do not completely understand the nature of matter, and the laws which govern its behavior.

Physicists do not believe that within the energy range now available there can be enough clues for us to piece together a complete theory of the nature of matter.

This conviction, together with our awareness of the value of exploration, our appreciation of the countless instances when unexpected, puzzling, and illuminating new observations have come out of accelerator experiments, is what motivates the quest for higher energies, for a microscope of higher magnification.

We can define a frontier where our current understanding ceases to make sense, and where the clues we need will have to be found. That frontier lies at energies of about one trillion electron volts for the collisions between quarks and leptons.

To put this in the language of microscopes, we need to resolve distances of less than a billionth of a billionth of a meter. That frontier of our understanding sets the scale of the Super Collider.

In addition, of course, whenever we explore new territory in particle physics, we share the excitement of the unknown with other adventurers. When we build a new instrument, we are like the astronomers looking out to a greater distance with new telescopes like medical researchers looking within the body in new ways.

There are always new phenomena to discover when you look where no one has been able to look before. And it is our confident expectation that in addition to the questions we now know we wish to answer, there will be new questions and new answers.

I would now like to pass on to a brief summary of what we intend to do with the budget request for next year. Four years of intensive research and development for the SSC project have led to a coherent and detailed plan to make the Super Collider available for experimentation in the mid-1990s.

The fiscal year 1989 budget request, which contains elements designated both R&D and construction, in DOE nomenclature, is matched to that long-range plan. The Super Collider is an integrated system that requires coordinated progress on many fronts to be realized.

Details of the budget request appear in the statement I have submitted for the record. But a few brief comments will serve to indicate the priorities as we see them.

To accomplish the work we have in mind, the fiscal year 1989 budget request includes $64 million in R&D operating funds for the development of technical systems for the collider ring and the accelerators that will make up the injector system.

The dominant element in this category is the development of superconducting magnets. In addition, also under the category of R&D, capital equipment has been requested in the amount of $16 million to support the R&D program for technical components, and also very importantly to begin R&D on the detector elements that

will be used to exploit the SSC. The R&D request thus totals $80 million.

The request for construction funds in the amount of $283 million includes many items that are not devoted to work at the eventual laboratory site. We plan to give high priority to items that concern technical readiness to proceed with construction once a site has been selected and delivered.

These include the procurement of long lead basic materials and tooling needed for industry to begin the production of magnet prototypes, engineering designs, and the development of technical components.

Planning for the Super Collider, which began in 1983, has been inspired by the scientific need for this new instrument to be available for experimentation in the mid-1990s. The conceptual design completed in 1986 and the ongoing R&D program have provided a sound technical foundation for the construction and utilization of the SSC.

The Super Collider is a bold project on a grand scale. We believe the rewards to fundamental science and to our Nation will be commensurate. We are ready to begin and we ask your support in this exciting enterprise.

Finally, to say that this can only be undertaken at the expense of other science is to narrow our vision of America's future in a most discouraging way.

Money spent on basic research is like the money spent on our children. Neither is the best investment for immediate financial return. Nevertheless, the world is unthinkable without children, and the future of our culture is unthinkable without basic research. Thank you very much.

[The prepared statement of Dr. Quigg follows:]

Statement of Chris Quigg
SSC Central Design Group
Universities Research Association

before

The Senate Subcommittee on Energy Research and Development

April 12, 1988

Mister Chairman, Members of the Subcommittee: Thank you for the opportunity to report to you on the Research and Development Program for the Superconducting Super Collider.

Physicists have just celebrated the three-hundredth anniversary of Isaac Newton's discovery of the law of gravity, a monument to human understanding of the physical world. Not only did Newton explain the elliptical orbits of the planets, but he also showed that the same force of gravity was responsible for the motions of the satellites of Jupiter and Saturn, the movement of the moon about the earth, the tides in the sea, and the fall of an apple from a tree. In other words, he recognized the *universality* of the Law of Gravity. That realization was perhaps Newton's most important and lasting contribution. It is an indication that the same laws of Nature hold at all times and in all places. This is the basis for our conception of the universe as an orderly, rather than capricious place—a universe we may hope to comprehend. The idea that in laboratories on Earth we can learn the physical laws that govern the structure of the Universe has had a profound effect on our civilization.

Elementary particle physics, the field that the Supercollider is designed to illuminate, is the science of the ultimate constituents of matter and the interactions among them. It seeks to answer two very simple questions: What is the world made of? and How does the world work? In common with other scientists, we hope that by exploring the basic laws of Nature, by finding and codifying laws that have a wide scope of applicability, we will better understand our universe and eventually have the possibility of putting that better understanding to humane and productive use.

Over the past two decades, the age-old struggle to describe and comprehend the nature of matter has been rewarded by a radically new and simple picture of Nature. This progress has been in large measure stimulated by experimental results from the particle accelerators at Brookhaven, Cornell, Fermilab, and Stanford in the United States, as well as from other machines abroad. We have learned that all matter in its diverse forms is assembled from a few basic building blocks called quarks and leptons. All known natural phenomena can be described in terms of a few fundamental forces acting among these basic constituents. These insights not only provide a framework for describing and understanding the world around us, but also elucidate the first instants after the creation of the universe.

However, as our conception of matter has become better established, we have become increasingly aware of its shortcomings. Since the late 1970s, the international scientific community has recognized that we do not completely understand the nature of matter and the laws which govern its behavior. Physicists do not believe that within the energy range now available there can be enough clues for us to piece together a complete theory of the nature of matter. This conviction, together with our awareness of the value of exploration, our appreciation of the countless instances when unexpected, puzzling, and illuminating new observations have come out of accelerator experiments, is what motivates the quest for higher energies, for a microscope of higher magnification.

We can define a frontier where our current understanding ceases to make sense, where the clues we need will have to be found. That frontier lies at energies of about *one trillion electron volts* for the collisions between quarks and leptons. To put this in the language of microscopes, we need to resolve distances of less than a billionth of a billionth of a meter.

The frontier of our understanding sets the scale of the Supercollider. In addition, of course, whenever we explore new territory in particle physics we share the excitement of the unknown with other adventurers. When we build a new instrument, we are like astronomers looking out to a greater distance with new telescopes, like medical researchers looking within the body in new ways. There are always new phenomena to discover when you look where no

one has been able to look before. It is our confident expectation that in addition to the questions that we know we wish to answer, there will be new questions and new answers.

Some Open Questions

Let me review for you in general terms some of the questions about Nature that are raised by our current understanding, but not answered. This will indicate the kind of initial exploration that we want to conduct with the Supercollider.

In the same way that, once having constructed the periodic table of the elements, our scientific ancestors asked themselves what made the elements behave according to certain patterns, we now want to know what determines the properties of the quarks and leptons. It is very well to be able to arrange the fundamental constituents in families and notice the relations between them, but how much more satisfying it would be if we could tell how much a quark or lepton will weigh, and what other attributes it will have. Must we be content to describe the patterns, or can we penetrate the mystery of their origins?

We have evidence for six quarks and six leptons, but we don't have any reason why there should only be six of each, or that there should indeed be six of each. How many of them are there?

There are many close similarities among the quarks and leptons. Indeed, mathematical consistency of the theory that brings radioactivity and electromagnetism together requires that there be a connection between quarks and leptons. For each set of quarks there has to be a set of leptons, and the properties of the quarks and the leptons must somehow meld together. How?

Are there even simpler and more fundamental things to be found inside the quarks and leptons? We recall that atoms, nuclei, and protons all looked fundamental in their turn. The observation of constituents within the quarks and leptons would give us a better understanding of matter, and the ability to construct a more complete picture of the world around us.

Are quarks and leptons the only fundamental constituents, or are there other kinds of matter that we haven't yet seen?

In addition, there are questions that have to do with the forces that act among the basic constituents. We have found a nearly complete connection between the weak interaction, which governs radioactivity, and electromagnetism. However, there is a fundamental difference between these interactions. Electromagnetism acts over very large—literally astronomical—distances. The weak interaction acts only over distances that are small even compared with the size of an atomic nucleus. What makes radioactivity and electromagnetism different and yet at the same time so similar?

Having seen the complete unification of electricity and magnetism in electromagnetism, and the partial unification of the weak and electromagnetic interactions, we may ask whether there is an underlying unity among all the fundamental forces.

Finally, as with the fundamental constituents, we may inquire whether there are new kinds of phenomena waiting out there—new fundamental forces that we haven't seen before.

Connections

Elementary particle physics does not exist in isolation, either as a scientific discipline or as a technological enterprise. Stimulation, information, and techniques flow both ways: from other activities toward elementary particle physics, and from elementary particle physics toward other activities.

The intellectual links to other sciences are strong. They grow more robust as our own field of elementary particle physics develops a greater unity and coherence. Quantum chromodynamics, a theory of the strong interaction between quarks, finds applications and some of its most probing tests in nuclear physics. We share with our colleagues in atomic, molecular, and optical physics an interest in tests of quantum electrodynamics and measurements of the fundamental properties of the electron, muon, and other basic particles. In the last decade, a

close working relationship has developed between elementary particle physicists and astrophysicists and cosmologists concerned with the evolution of the universe. The laws of Nature uncovered in the study of elementary particle physics are essential ingredients in imagining what the early universe must have been like. Astrophysical relics from those early moments when the universe was small, hot, and dense test our ideas about the fundamental interactions at higher energies than we will ever be able to attain with accelerators.

Much of our insight into the nature of complex systems built up from simple components and simple rules comes from the science of condensed matter physics. We share with this field many theoretical tools, and an interest in hidden symmetries and changes of phase, or state. Traditional connections between theoretical particle physics and mathematics are once again in great ferment, with the recognition of the common ground shared by fibre bundles and the gauge theories of the fundamental interactions, with the rich interchange between new mathematics and superstring theories, and with the emergence of the interdisciplinary study of complex (nonlinear) systems called "chaos."

The health of these other fields of basic research is essential to the vitality of elementary particle physics and the advance of basic research requires vigorous progress in elementary particle physics.

Elementary particle physics also enjoys strong links to new technologies. A few examples will serve to illustrate how benefits flow both to and from elementary particle physics. Synchrotron light sources, the offspring of accelerator science, now find routine application to materials research, biomedical studies, and the fabrication of electronic components. The high-density semiconductor devices manufactured in this way are important elements of the detectors used in elementary particle physics experiments. Advances in the capability of the niobium-titanium superconducting cable so essential to the Supercollider (and to the availability of magnetic resonance imaging devices for medical diagnosis) have resulted from a purposeful collaboration among accelerator builders, university-based metallurgists, and industrial firms. The demands of elementary particle physics have spurred the development of

fast electronics and new computer architectures to handle high data rates, and to make the rapid decisions required by modern detectors. Theorists, experimenters, and accelerator builders are quick to take advantage of new capabilities spawned elsewhere in networking, the application of remote local intelligence to process control, and engineering design. The components of our detectors, including semiconductor devices, tracking chambers, and liquid calorimeters, also provide fruitful interchange with other researchers, manufacturers, and developers of technology in fields such as medical imagery.

Budget Summary

Four years of intensive research and development for the SSC project have led to a coherent and detailed plan to make the Supercollider available for experimentation in the mid-19'0s. The FY 1989 Budget Request, which contains elements designated both "R&D" and "Construction" in DOE nomenclature, is matched to that long-range plan. The Supercollider is an integrated system that requires coordinated progress on many fronts to be realized. Details of the Budget Request appear in the final section of this submission, but a brief narrative will serve to indicate the priorities as we see them.

For the past two years, particular emphasis has been placed on development of full-length (17-meter) superconducting dipole magnets for the collider rings and of the advanced niobium-titanium superconducting cable used in their fabrication. Thanks to a collaborative research effort with U.S. industry, excellent gains have been realized in manufacturing characteristics of superconducting wire, key factors in the cost of the magnets. Fabrication and testing of several different styles of superconducting magnets confirm this technology as a mature, well-understood basis for the SSC.

The systems aspects of the SSC have also undergone considerable development during the R&D program. Accelerator calculations, studies, and experiments have yielded large gains in our understanding of the behavior of circulating beams. This has led in the past two years to a series of small but significant improvements in the design of the collider. Neutron irradiation

tests indicate that commercially available cold current-bypass diodes meet SSC radiation-resistance specifications, opening the way for a simplified passive magnet quench protection scheme. An elegant cryostat design has reduced heat leakage into the cooled SSC magnet system by a factor of six compared with the Fermilab Tevatron, and the effects of synchrotron radiation on the vacuum in the beam pipe are understood to pose no operational threat. All of these achievements indicate that the Supercollider will cost considerably less to build and operate, on a relative scale, than the Tevatron.

During the coming year the SSC R&D program will continue investigations of superconducting magnet fabrication, systems engineering, detector technology, and operational aspects of the accelerator design. A magnet industrialization program, to begin in 1988, will transfer SSC magnet technology to industry. This will lead to the establishment of commercial sources of superconducting accelerator magnets. The Central Design Group expects to finalize a development model magnet that meets all specifications in the first half of 1989. Enhancement of industrial techniques for fabrication of superconducting cable will continue. A test-bed incorporating full-length SSC model magnets will be commissioned as an operational proving ground of cryogenic, electrical, and vacuum systems for the collider. Accelerated life testing of pre-prototype SSC dipoles will be inaugurated to explore potential failure modes and mechanisms. Advanced conceptual designs for the four accelerators in the injector chain will be emphasized, and prototyping of technical components will commence. To meet the experimental challenge of the SSC's high event rate and the large number of particles created in each proton-proton collision, new detection techniques will be developed, and established methods improved. In the realm of experimentation, the involvement of the high-energy physics community at large will continue through workshops, task forces, and summer studies.

To accomplish this work, the FY 1989 request includes $64 million in R&D funds for the development of technical systems for the collider ring and the accelerators that make up the injector system. The dominant element in this category is the development of superconducting magnets. Capital equipment has been requested in the amount of $16 million, in support of the R&D program for technical components and to begin R&D on detector elements. The request for

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construction funds in the amount of $283 million includes many items that are not devoted to work at the eventual laboratory site. We will give high priority to items that concern technical readiness to proceed with construction, once a site has been selected and delivered. These include the procurement of long-lead basic materials and tooling needed for industry to begin producing magnet prototypes, engineering designs, and the development of technical components.

A Brief History of the SSC

Although the scientific need for a step to much higher energies was already apparent, in broad terms, a decade ago, it was necessary to develop the technical means to take that step. Formal studies of the feasibility of a multi-TeV accelerator began with workshops sponsored by the International Committee on Future Accelerators at Fermilab in 1978 and at CERN in 1979. Various possibilities for very high energy accelerators, including proton-proton colliders at tens of TeV per beam, were examined. The idea of the SSC began to take shape at the 1982 Summer Study on Elementary Particle Physics and Future Facilities organized in Snowmass, Colorado, by the Division of Particles and Fields of the American Physical Society. The SSC initiative was followed up with workshops on accelerator and detector issues held during 1983 at Cornell University and at the Lawrence Berkeley Laboratory.

Encouraged by the successful operation of the Fermilab Tevatron (the first superconducting synchrotron), where protons have been boosted to energies of nearly one trillion electron volts, a High Energy Physics Advisory Panel (HEPAP) Subpanel on Future Facilities recommended "the immediate initiation of a multi-TeV, high-luminosity [high collision-rate] proton-proton collider project with the goal of physics at the earliest possible date." This recommendation was transmitted to the Department of Energy with the unanimous endorsement of HEPAP in July of 1983. The DOE responded by programming funds for preliminary R&D for the SSC in the fall of 1983.

In December 1983, the DOE and the Directors of the United States high-energy physics laboratories chartered a Reference Designs Study. The goal was to produce example designs of a proton-proton collider that would yield about 100 million collisions per second with energy of 20 TeV per beam. Three different approaches were investigated, and it was found that any of them could form the basis of a technically feasible SSC, at a cost of approximately three billion (FY 1984) dollars.

Early in 1984, the Department of Energy contracted with Universities Research Association, a consortium of 66 leading research universities, to direct and coordinate the SSC research and development program. For this purpose the SSC Central Design Group was established, accepted the hospitality of the Lawrence Berkeley Laboratory, and began work on a non-site-specific conceptual design. Participants included Brookhaven National Laboratory, the Fermi National Accelerator Laboratory (Fermilab), Lawrence Berkeley Laboratory, the Texas Accelerator Center, and experts from many laboratories and universities in Canada, Western Europe, Japan, and the United States. American industrial firms also played an important role.

Approximately 8000 dipole magnets will generate the magnetic field that keeps the protons circulating around the SSC collider rings. An early goal was, therefore, to determine the basic dipole magnet style. In September 1985, after several different concepts had been developed and studied, a high-field superconducting magnet in which the field is defined by the placement of the superconductor was adopted as the basis for the conceptual design.

Simultaneously, all accelerator and conventional systems (other than detectors) were defined and assessed. The completed Conceptual Design Report (CDR) details the scientific, technical, and cost aspects of the project. Since its publication in March 1986, the Conceptual Design Report (SSC-SR-2020) has been extensively reviewed by independent experts. This document has served as the basis for continuing SSC R&D.

Description of the SSC

The SSC is a chain of accelerators, linked to boost barely moving protons to nearly the speed of light. The particles reach increasingly high energies in a cascade of four accelerators that make up the injector, from which they are fed into the collider ring.

The collider ring is the heart of the SSC. In this underground oval tunnel, 10 feet in diameter and 53 miles in circumference, two beams of protons are accelerated in opposite directions. Superconducting electromagnets in the collider ring guide each proton beam through a stainless steel pipe about one and a quarter inches in diameter. Magnets curve the paths of the beams, making it possible to build the accelerator in a loop, rather than a straight line, and thus realize substantial economies by repeatedly using the same components to accelerate and manipulate the beam. The protons travel in compact bunches, with eight billion protons in each bunch. Each second, the fifteen thousand bunches that fill each beam pipe make three thousand laps around the ring. On every lap, a pulse of radio waves boosts the energy of each bunch, until they reach their final energy—twenty trillion electron volts. The circulating beams are maintained at that energy and focused into head-on collisions where detectors can record particles produced from the collision energy.

Superconducting magnets are used because they make possible substantial savings. At low temperatures, certain materials become superconductors, in which electricity is able to flow without resistance. Such a material, an alloy of niobium and titanium, will be used to fabricate the coils for the SSC magnets. The magnets will be operated at the temperature of liquid helium, about 4 degrees above absolute zero. Once these magnets are turned on, electric power is only required to drive the refrigerators that keep them cold. This is less than one percent of what would be needed to power conventional electromagnets built from copper cable.

In addition, superconducting magnets can create magnetic fields several times stronger than conventional magnets, thus confining the protons to a tighter orbit and reducing the ring's perimeter.

An important benefit of the cryogenic system is its contribution to maintaining the vacuum in the beam pipe. If the protons bumped into air molecules as they traveled around the ring, they would lose energy and be deflected from their proper orbit. The liquid helium for the superconducting magnets bathes the outside walls of the beam pipe and lowers its temperature. Any stray molecules inside the pipe tend to adhere on the cold inside walls of the beam pipe, out of the protons' way, just as a damp finger sticks to an ice cube.

At the interaction regions the two beams are steered into head-on collision. Experimental apparatus at these points is designed to provide two kinds of information: to identify the particles produced in the collisions by measuring their properties, and to determine as precisely as possible the path each particle followed. Although similar in concept to existing detectors, SSC detectors will be physically larger and more complex. Using a large array of state-of-the-art components, international teams of physicists and engineers will design, construct, install, and operate the experimental apparatus at the SSC.

Precise control is necessary to coordinate the five accelerators that make up the SSC chain, and to ensure that the magnets, power supplies, refrigerators, and control devices function in harmony. Special instruments monitor these systems and relay their observations electronically to a distributed complex of computers.

The R&D Program

For the past two years, the Central Design Group has coordinated a multi-fronted assault on the challenge of refining a technically assured, economically optimal conceptual design. A team of scientists, engineers, and technicians from around the country has been engaged in bringing the idea of the SSC to reality. I am pleased to offer a brief summary of some of the highlights of this work.

The Accelerator. Accelerator designers associated with the SSC have had several important tasks: refinement of the design of the accelerator itself, the detailed specification of

performance requirements for technical components, and the development of algorithms to monitor and control the performance of the accelerator complex.

All the way around the 53-mile long tunnel, the SSC beams must be accurately guided along the center of the beam pipes. The magnets must be carefully laid out to achieve this with optimum beam quality and cost. We have now designed a pattern for the magnets that confines the beam into a cylinder 0.068 centimeter in diameter, well within the aperture of the beam pipe.

More sophisticated optics are required to focus the beams into even tighter bunches at the collision points. To produce the desired high collision rate, beams at the collision points are compressed into cylinders smaller than a human hair. A special modular design has been devised that not only squeezes the diameter of the beams but is also extremely flexible, allowing a wide range of collision conditions appropriate for different types of experiments.

An important design decision for a circular accelerator is the size of the magnet aperture through which the beam travels. The cost of building magnets increases with the diameter of the required uniform magnetic field, but high magnetic field quality, which determines beam stability, becomes more difficult to attain as the aperture is decreased. Accelerator physicists at the Central Design Group first developed detailed theoretical tools—both numerical and analytical—to examine a range of possible apertures. They then tentatively chose a beam pipe with an internal diameter of 3.3 centimeters as the most cost-effective for the SSC.

To check the predictive power and accuracy of the theoretical calculations, and to establish criteria for evaluating the SSC aperture, a team of accelerator physicists from the Central Design Group, CERN, Cornell University, Fermilab, and SLAC has been conducting a series of experiments at the Fermilab Tevatron. In a typical aperture experiment, the circulating beam is given a sudden kick that causes it to oscillate about its equilibrium orbit, while special sextupole magnets of known strength are powered to mimic the SSC environment. A pair of beam position monitors registers turn-by-turn beam position data after the fast kick for up to a few hundred thousand turns.

When the experiments are completed, we will be able to confidently set the magnet aperture. Any changes that might be required would be small and would not have a significant cost impact. The knowledge gained in accelerator physics, particularly nonlinear dynamics, should prove valuable for all circular accelerators and for other particle beam devices.

Accelerator Systems. Several technical challenges posed by the collider's superconducting magnet support systems have been addressed in the past two years. Results of experiments, calculations, and studies increase our confidence that commercially available components can meet many of the requirements of these systems.

The SSC will have the world's largest helium refrigeration system. The superconducting magnets must be maintained at the chosen operating temperature of 4.35 degrees from absolute zero under a variety of operating modes and conditions.

To allow construction, installation, and commissioning to be performed in parallel, we have designed a system of cryogenic units capable of independent operation but interconnected for redundancy. The present design calls for ten units for the collider ring, plus an eleventh to service the injector. Each sector requires a cryogenic refrigeration capacity of the equivalent of about 7500 watts at 4.35 kelvins, within current industrial practice.

However, the SSC cryogenic system presents a significant challenge in process control. It must not only produce and deliver the cooling required for normal operation of the collider, but also accommodate such transient thermodynamic conditions as cooldown, magnet quench, and beam acceleration. In addition, it must function reliably to meet the design requirement that beam be available for experiments at least 80 percent of the time.

With help from industry, the Central Design Group has undertaken two computer simulations of the SSC's cryogenic system. In the first, refrigeration plant concepts are investigated by static process simulation: a base operating plant flow sheet was developed and studied in a variety of operating modes. More recently, we began to model the behavior of the cryogenic system as a whole: the refrigeration plant design is simplified, and the behavior of

transient conditions in one cryogenic sector of the collider and the cryogenic interactions between sectors can be analyzed. Both simulations are important for a final engineering design. Modeling confirms the adequacy of initial design concepts, aids instrumentation and control systems design, and assists the task of writing specifications. It also provides a tool for judging design proposals and helps assure that a self-consistent set of specifications has been produced.

The Detectors. To answer our questions about the structure of matter, the SSC must have a high collision rate. About 100 million proton-proton collisions will occur every second at the SSC, some of which will produce hundreds of particles. Identifying these evanescent products requires measuring their energies and directions.

To achieve this goal, many-layered detectors surround the interaction region. Typically, closest to the collision point, a detector with good spatial resolution tracks the many closely spaced particles emerging from the collision. Another layer measures the curved paths that electrically charged particles follow in a magnetic field. The energy and direction of both charged and neutral particles or groups of closely spaced particles is measured by the outer layers. In every layer, high spatial resolution is important to enable different products to be distinguished.

Storing and studying the data from every collision would, of course, be impossible. Sophisticated electronics and computer systems filter the data and discard phenomena that are already well understood, just as in daily life we pay closer attention to unusual occurrences than to the mundane. Only a handful of interesting collisions per second are recorded permanently.

The SSC's high event rate, the large number of tracks per event, and the need to resolve the tracks of different particles that have similar energy and direction present a challenge to detector technology. To meet this challenge, new detection and information-processing techniques must be developed or older methods improved.

Detectors for the SSC must be colossal to trap the very high energy particles that the SSC will create: an SSC detector might weigh as much as 50,000 tons and be as tall as a

five-story building. Despite this enormous physical scale, research and development of detector technology can be carried out by small groups of physicists and engineers at universities and laboratories. Productive collaboration with industries involved in silicon detectors, advanced electronics and computing, scintillating materials, and many other areas is vital.

At present, detector research includes the development of high-speed, two-dimensional silicon detectors (pixel devices) and the associated "smart" readout electronics that record only selected data; custom integrated circuits for low-noise, high-speed applications; new radiation-resistant scintillating materials; large-volume, high-field superconducting magnets for experiments; and new large-scale methods for precise measurement of particle energies and directions by calorimetry. Initial work is being pursued in more than a dozen universities and in five high-energy physics laboratories across the country, involving collaborations with scientists in Canada, Japan, and Europe. As the opportunity for university and laboratory involvement is large, the next two years will see a substantial increase in such efforts.

Magnets. The 7680 superconducting dipole magnets form the principal technical system of the SSC and have been a primary focus of the preparatory R&D program. A cross-section of the magnet, through one of the support points, is shown in Figure 1. A shaped coil made from multi-strand superconducting cable is tightly clamped in laminated collars, which are enclosed in a yoke of magnet steel. This cold mass is contained within a thin, stainless steel skin, which forms the helium vessel. The 55-foot-long SSC magnet is similar in concept to the successful 21-foot-long Fermilab Tevatron dipole and to the 30-foot-long magnet for the HERA proton ring now under construction in the Federal Republic of Germany. It is our understanding that a similar choice has been made by Soviets for their UNK collider project now under way near Moscow.

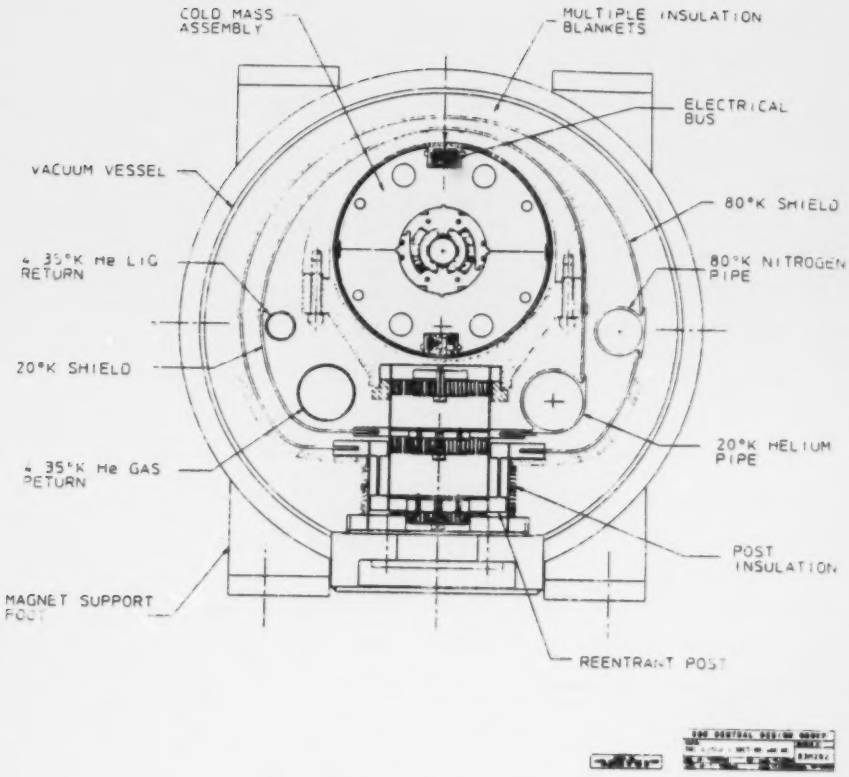

Figure 1. SSC collider dipole cross section.

The magnet specified in the Conceptual Design Report was based on test results of six highly successful 15-foot-long cold mass models. Scientific and engineering projections of improvements in the superconducting material and cryogenic insulation technology were considered as well. An iterative, systematic development program was envisioned, which would lead to commencement of full-rate industrial production of the dipoles in 1992.

The heart of the magnets is the coil assembly made from cables of composite strands containing thousands of filaments—each one-sixth the diameter of a human hair—of an alloy of niobium and titanium embedded in a matrix of copper. The performance capability of the magnets depends on the amount of current the cable can carry. This, in turn, depends upon the metallurgical processes used to form the alloy into wire and cable. Substantial improvements to this technology have been achieved over the past four years as a result of a collaboration of interested American firms, universities, and national laboratories. This very sophisticated technology involves metallurgical manipulations of the material at the atomic scale. The increases in current-carrying capacity shown in Figure 2 have met our goals and made American-manufactured superconducting magnet cable the best in the world. R&D in the next year will focus on large-scale process optimization to maximize the yield and minimize the cost.

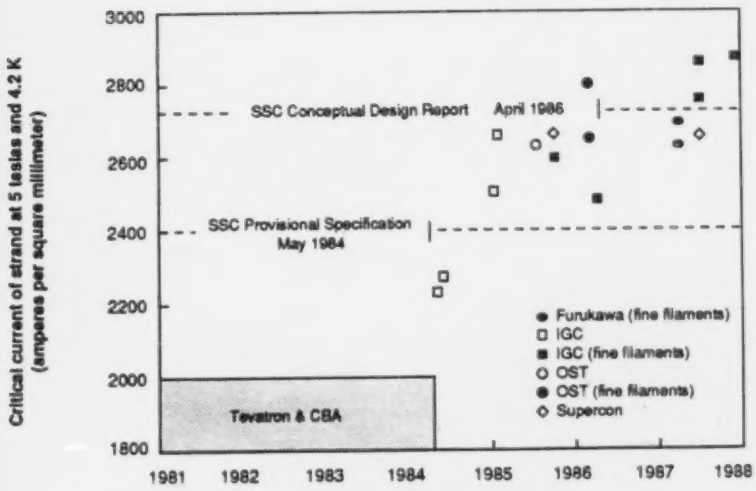

Fig. 2. NbTi superconductor for SSC magnets.

The cryostat, which encloses the magnet core and maintains it at 4.35 degrees above absolute zero with very low refrigeration power, is also shown in Figure 1. It consists of an outer shell, surrounding layers of temperature-controlled shields and multi-layer insulation blankets. These, in turn, encircle the cold mass and the piping that carries the liquid helium coolant to and from the magnet chain. An important feature is the re-entrant post assembly that supports the weight of the magnet and fixes the magnet's position, while minimizing heat transfer, thanks to recently developed high-strength composite materials. Engineering calculations made at the time of the CDR led us to project an improvement in unit heat leak of better than six times over the technology then available. This has been achieved, and the cryostat is now ready for industrial engineering and manufacturing design.

System requirements for the magnets include not only achievement of the design magnetic field strength, but also other properties necessary for accelerator service. The magnetic field must be aligned with great precision and be very uniform. The electrical insulation must be sufficient for all possible operating and upset conditions. The cold superconducting coil must be thermally isolated from the outside world. Th interior temperature must be held below the deformation temperature for the structural materials under all operating and upset conditions. The number of training cycles needed for a newly made magnet to achieve full operating strength must be small, and that training must be retained when the magnets are warmed. Ultimately, the manufacturing process must be optimized for a high rate of industrial production.

A three-pronged approach to achieving these design goals was envisioned early in the conceptual design phase and is now in process:

a) Tests of short model magnets. For study and verification of improved superconducting materials and cold mass assembly details, a series of short models of full cross-sectional dimensions has been built and tested. All needed operating characteristics have been met or exceeded. For example, using the newly developed cables, field strengths of more than 7.2 teslas have been reached at the SSC design operating temperature. This exceeds the SSC target of 6.6 teslas. To test the ability of the basic mechanical structure of these magnets to

withstand the magnetic forces encountered in operation, the test temperature has been lowered to enhance the current-carrying capacity of the superconductor. Under these circumstances, fields of greater than 9 teslas have been achieved.

b) Tests of full-length model magnets. Concurrently, full-length prototypes have also been constructed and tested, and many of the required properties have been met or exceeded. The first two of the four now-complete prototypes achieved 85 percent of design strength. The second two prototypes achieved design strength, albeit requiring too many training cycles. One of them exhibited a breakdown of electrical insulation. All other requirements were met. This information is summarized in Table I. Measurements to date indicate that the differences between short and long magnet performances are due to the redistribution of the forces brought about by increasing the length. Feedback from measurements of these first prototypes is now put into the program and instrumented prototypes that address this redistribution of forces will soon be tested as the next step in the planned program. Three new prototypes are essentially ready for test and two more are in an advanced stage of preparation.

Table I. Collider Dipole R&D Status.

REQUIREMENT	STATUS	
	Short Models*	Full-length Models†
Design Field	OK	1&2 NO; 3&4 OK
Field Alignment	OK	OK
Field Uniformity	OK	OK
Electrical Insulation	OK	Must be improved
Heat Load	OK	OK
Max. Quench Temperature	OK	OK
Training Behavior	OK	Needs improvement
Training Retention	OK	Not checked
Manufacturing Process Optimization		Planned for initiation in FY 1989

* 25 models complete, 1 to 4.5 meters in length
† four 17-meter models complete; 66 required to complete planned program

c) Transfer of technology to industry. Subassemblies and components of the magnets are already supplied by industry, but commercial firms must be involved in engineering and manufacturing design and in the assembly of the complete magnets. A three-phase program for accomplishing this goal and the actual manufacture of the magnets in industry has been laid out. The first phase, which we hope can be well along by the end of this year, will disseminate all available technical information to industry and engage industry's intellectual resources in the magnet technology. The second phase, which we hope to have well under way next year, will result in pre-production prototype magnets of industrial design and provenance. This will lead to full-rate collider dipole production—the final phase of the industrialization program—in 1992.

FY 1989 Budget Request

Plans for the development of collider magnets form part of a broader program of work on technical systems that come under the rubric of both "R&D" and "Construction" in DOE nomenclature. The SSC is an integrated system. An injector composed of four cascaded accelerators feeds the collider ring. The injector complex requires accelerator physics studies, as well as some component and subsystem R&D. In addition to the magnets, the collider ring requires cryogenic, acceleration, and control systems. They, too, are ready for R&D and engineering design and development. Engineering studies of the conventional system requirements for these technical systems are also required.

Another important item that needs to be started now is R&D on detector elements that will be needed for scientific exploitation of the SSC. Accordingly, a budget for this work has been included in the FY 1989 request. These items are listed, together with the requested resources, in Table II.

Table II. FY 1989 SSC R&D Program Plan.

	M$
Accelerator Physics	3.0
Injector Systems	3.5
Superconducting Magnet Development	44.0
Collider Technical Systems	5.5
Conventional Systems Development	1.0
Detector Development	7.0
Total R&D (Operating)	64.0
Accelerator Equipment	11.0
Detector Equipment	5.0
Total Equipment	16.0

The magnets occupy a prominent position, commensurate with their technical importance in the overall scheme. Table III shows a further breakdown of the magnet component of the R&D request. Table IIIa outlines how we plan to use the requested resources.

Table III. FY 1989 SSC Superconducting Magnet R&D Program Plan.

	M$
Collider Dipoles	18.2
Industrialization Program	8.0
Collider Quadrupoles	2.3
Collider Spools	2.0
Collider Interaction Region Quadrupoles	1.5
Collider Special Magnets	3.0
Systems Integration	2.0
Cell Tests	2.0
Accelerated Life Tests	2.0
High-Energy Booster (Injector) Magnet Systems	3.0
TOTAL	44.0

Table IIIa. FY 1989 SSC Superconducting Magnet R&D Program Plan.

• Collider Dipoles	
– Fabricate, Test, and Analyze	10 Short Models
– Fabricate, Test, and Analyze	28 Full Scale Models
• Industrialization Program	
– Award Contracts for Phase II Industrialization	
– Begin Detailed Production Designs for Dipoles	
• Collider Arc Quadrupole	
– Fabricate, Test, and Analyze	5 Short Models
• Collider Spools	
– Fabricate, Test, and Analyze	3 Full Scale Models
• Collider Interaction Region Quadrupoles	
– Fabricate, Test, and Analyze	2 Short Models
• Collider Special Magnets	
– Fabricate, Test, and Analyze For Vertical Dipoles, Dispersion Suppressor Region Quads, and Utility Region Quads	~7 Development Models
• Systems Integration	
– Design, Test, and Analyze Component Interconnections, Systems Tests, and Systems Operations	
• Cell Tests	
– Complete Facilities Preparations and Begin Tests of Integrated Collider Components (Dipoles, Quadrupoles, and Spools)	
• Accelerated Life Tests	
– Complete Technical Facilities and Execute Life Cycle Tests of Full-Scale Magnet Models	
• High-Energy Booster Magnet Systems	
– Complete Model Dipole and Quad Designs	
– Fabricate and Test 2 Dipole Models and 2 Quadrupole Models	

Table IV displays the part of the program request that comes under the rubric of "Construction." Note that the great majority of the items concern technical components that are not site-specific and do not require that the work be done at a particular site. For example, the first item concerns collider magnets and would provide for the long-lead basic materials and tooling needed for industry to begin producing prototypes. Other items are for engineering designs and development of technical components. Table IVa indicates the uses to which we plan to put the requested resources.

Table IV. FY 1989 Construction Plan Obligations (M$).

	TOTAL PROGRAM	TECHNICAL SYSTEMS (Non-Site-Specific)
1. TECHNICAL SYSTEMS — COLLIDER	67.7	67.7
1.1 S.C. Cable Tooling	1.5	
1.2 Superconductor	13.7	
1.3 Magnet Tooling	29.3	
1.4 Magnet Material	15.6	
1.5 Cryo – Refrigerator	7.6	
2. PLANNING AND DESIGN	31.0	31.0
2.1 Tech Design Groups	22.5	
2.2 Planning – Management and Support	8.5	
3. TECHNICAL SYSTEMS — INJECTOR	56.8	56.8
3.1 Source and Linac	22.3	
3.2 LEB	14.9	
3.3 MEB	19.6	
4. CONVENTIONAL SYSTEMS DESIGN	16.2	7.0
5. CONVENTIONAL SYSTEMS ACTIVITY	89.0	
5.1 Site Preparation	28.9	
5.2 Electrical Distribution	47.5	
5.3 Electrical Procurement	12.6	
6. PROJECT SUPPORT	22.3	22.3
6.1 Support Equipment	20.8	
6.2 Support Facilities	1.5	
SUBTOTAL non-site-specific technical systems		184.8
TOTAL	283.0	

Table IVa. FY 1989 SSC Construction Program.

A. Technical Systems	
• General	– Complete organizational structure – Mobilize technical design staff – Coordinate design plan with R&D program – Establish monitoring and reporting systems
• Linac	– Start detailed design of major systems – Start fabrication of linac sections, RF systems, and control systems
• 8 GeV Synchrotron (LEB)	– Start detailed design of major systems – Start fabrication of magnets, power supplies, RF systems, and control systems
• 100 GeV Synchrotron (MEB)	– Start detailed design of major systems – Start fabrication of magnets, power supplies, vacuum, and RF systems
• 1 TeV Synchrotron (HEB)	– Start detailed design programs for magnets, power supplies, cryogenics, vacuum, and RF systems
• Collider Ring	– Start design programs for magnets, cryogenics, and power supply systems – Start fabrication and procurement of superconducting cable tooling, superconductor cable, and magnet materials – Contract for magnet test facility
B. Conventional Systems	
• General	– Mobilize design staff – Establish systems specifications – Coordinate with technical requirements – Initiate site preparation work
• Design	– Initiate detailed design for utility systems, roads, injector system structures, and first sector of the collider tunnel
• Construction	– Award contracts for site preparations – Initiate long lead procurements for power and utility systems
C. Project Support	
• General	– Procure equipment required in support of the construction program – Provide facilities to support design effort

The plans presented for 1989 fit into a broader plan to realize the SSC under the resource profile put forward by the DOE. A summary schedule of milestones and technical activities is shown in Figure 3. Orderly progress on the intimately interconnected paths of sequential technical activities requires a strong start on the essential engineering now.

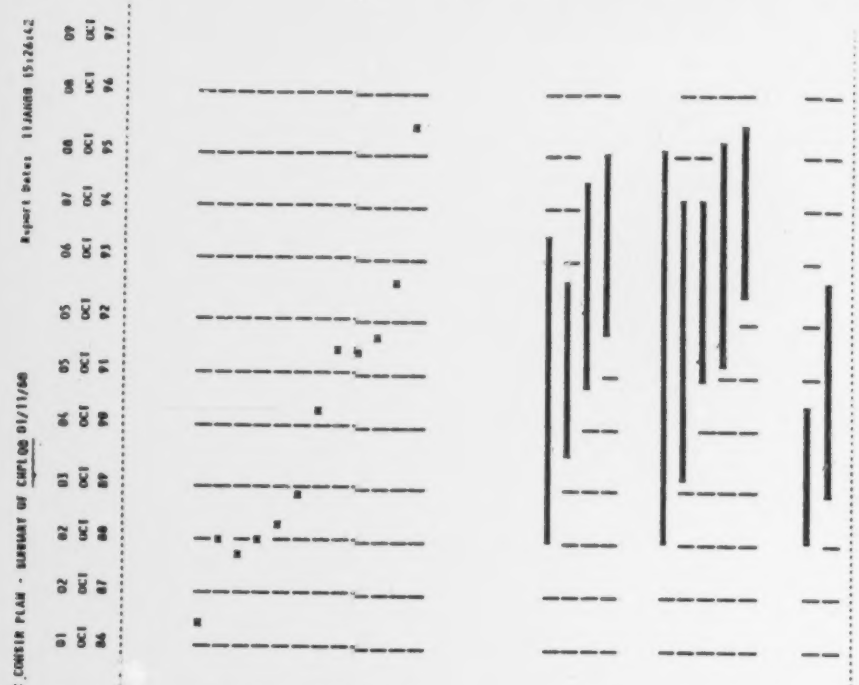

Figure 3

Conclusion

Planning for the Supercollider, which began in 1983, has been inspired by the scientific need for this new instrument to be available for experimentation in the mid-1990s. The Conceptual Design, completed in 1986, and the ongoing R&D program have provided a sound technical foundation for the construction and utilization of the SSC.

The Supercollider is a bold and innovative project on a grand scale. The rewards to fundamental science and to our nation will be commensurate. We are ready to begin, and we ask your support in this exciting enterprise.

That concludes my prepared statement. I will be pleased to answer your questions.

Senator FORD. Thank you, Doctor. Would Dr. Fisher, Dr. Brinkman, and Dr. Sessler return to the table? Dr. Lederman, excuse me. You see his hair? When he started working with the government his hair was black.

Senator Domenici will be required to go to the floor shortly, and so I am going to yield to him for some questions of the panel. And then I will have some questions of my own. Senator Domenici?

Senator DOMENICI. Thank you very much, Mr. Chairman. Might I ask all of you for your impression of this? And I really do not want a lengthy answer unless you feel totally compelled.

With reference to basic research, both as a tool for discerning new truths, and as a tool and instrument for educating minds, is the United States still premier in the world, in your opinion?

Let us start right over here. Do you think we still are?

Dr. SESSLER. Yes, I think we are. Other nations are very good. But I think we are, on balance, still in the lead.

Senator DOMENICI. We are still number one. What about the rest of you?

Dr. WOJCICKI. I would say we are. But I think the other nations are catching up very, very fast. If you look at all the trends, unless we really make a larger effort, we will not be in that position at the turn of the century.

Dr. FISHER. I think we are, but not in all areas. And I think in condensed matter there are serious worries in that respect.

And I would cite, for example, the neutron scattering facility near Grenoble as something that makes one really worried.

Senator DOMENICI. Dr. Quigg?

Dr. QUIGG. I believe we still are. Many other countries are now doing first-rate science. It requires us to continue to have the confidence to think that we can be number one.

Dr. BRINKMAN. I think we are too, but I also agree with Dr. Fisher that there are selected areas in which we are not.

Dr. LEDERMAN. Well, I have to be a minority. I think we are not, and the reason is that the slopes are different.

I think if you look at the slopes in Japan and Germany, and even the slopes in the Soviet Union now, they are very remarkable. And maybe technically by literature counts and so on we are up there, but the slopes to me are very depressing.

Dr. FISHER. I would certainly second that very strongly.

Senator FORD. Why is it they are always trying to steal our technology and want to get all of our stuff into their country then? Dr. Lederman?

Dr. LEDERMAN. I am not sure that is true anymore. That was true——

Senator FORD. Recently, we just had a 60 Minutes on it, and Congress is worried that this technology is going to other places. Of course, it was from Japan.

Dr. BRINKMAN. I do not think you really mean the Soviet Union. Japan and Western Europe are surely the main competitions, it is their slopes that are really the thing to be concerned about. Not the Soviet Union.

Senator DOMENICI. Frankly, I could not expect such a distinguished panel to be unanimous. But I think it is pretty significant that we are five to one on that premise.

Frankly my own view is that at this point in time the best evidence of the fact that we are still superior in basic knowledge is the fact that more countries want their young people to come here to get doctorates and advanced degrees than any other country in the world.

And that is from everywhere. Developed countries with the few talented people that they are able to pull to the top of the pot, plus the very developed countries, they still want them to come here.

So I had already arrived at my own conclusion on that premise, not that it means anything about SSC. But I think we should start with it. Then I would like to quickly ask the question, and I would like a very simple answer if you could please.

Why if we are, and since one does not think we are, he need not answer, why are we still number one?

Dr. SESSLER. Because of the investment that we have made through the years, which has allowed us to attain that position. Especially since World War II.

We have had a large buildup of basic research, and that has resulted in students, investigators, and instruments which has allowed us to get to this stage.

Senator DOMENICI. And would you add to that because we have had great diversity of activity, not centralized but going on in the purest sense of science with many scientists just free out there doing things?

Dr. SESSLER. Certainly I would agree with that.

Senator DOMENICI. Why do you think we are?

Dr. WOJCICKI. I think I have very little to add to that.

I think we have over the last 40 years created a situation that was very attractive. We were able to attract a lot of top-notch people, especially after World War II, that I think really helped in this.

Senator DOMENICI. Can we just kind of agree generally that that is the case?

Dr. FISHER. No. I would like to point out, picking up your last point, Senator, that the last three Nobel Prizes I referred to, coming from small science and condensed matter physics, were all three not to scientists in this country.

I think all three cases, many of the scientists, the senior ones had been trained here. But these Nobel Prizes went to small science work done outside the United States.

I think one of the important features has been referred to, but I would like to stress it. It was made by you yourself. Namely the openness that we had in this society when people could spread around.

Twenty years ago when I went to Cornell in condensed matter physics, it was not number one. Now it is considered to be one of the 12 which you always go to. It was not then.

I would like to see that preserved and the opportunities for young people to be allowed to develop.

Senator DOMENICI. Anything further? You all agree with that?

Now, let me ask, do any of you happen to know what institution within the Free World has control of the most scientists at the master's and Ph.D. level? Since we are decentralized, nonetheless there is some centralization. Do any of you have an idea?

Well, let me tell you who it is. It is the Department of Energy, strange as that may seem; 22,500 scientists are under the direct or indirect jurisdiction of the Department of Energy, because of Lawrence Livermore, Sandia, Fermi. So I just want to chalk that one up as a premise here for a moment, then we will go to something else.

Now, let me ask you, there was some alluding here to the National Science Foundation. Might I ask, is the National Science Foundation in your opinion doing a good job? And is it merely a question of them not getting adequate resources to cover the kinds of projects and innovative approaches to science today?

Dr. BRINKMAN. I think that over the years the National Science Foundation has done a good job.

I think there have been some hiccups in the last year or so, especially in funding in the material science division, that has been referred to and alluded to earlier, that have caused major concern in the community.

And so I think overall if you judge the long-term behavior, the National Science Foundation has done an excellent job.

It is very much stressed right now because of a commitment it made a number of years ago that it would take up engineering research. As you may remember, there was a debate about a national engineering foundation, and that was not done.

And so it has drained off a fair bit of money from the basic research into engineering research. And that has caused a lot of stress in the National Science Foundation. But I think that is a natural stress that has to be——

Senator DOMENICI. Well, let me suggest to you that the National Science Foundation is not getting the money they need, not because there are not great supporters for National Science Foundation, not because this President has not asked for substantial increases, but because, just as I asked you the question a while ago about DOE, it happens to find itself in the appropriation process of the United States in a very strange, strange category.

It is found in an appropriations bill called HUD and Independent Agencies. And if you would look into the bowels of that bill, you will find that it has two interesting competitors that have nothing to do with science.

Aside from NASA, which does have something to do with it, it has the veterans hospitals of the United States and the housing programs of the United States. And you might guess what happens.

When they are allocated an amount of money, and many assume they are going to increase NSF, they cannot find it within their set of priorities to put the money there, because the pressure from two very, very important functions of government that have very little to do with science.

And so if you want to know really why, that is why. Because whenever they need an extra couple of billion, so does the Veterans Administration and so does housing. A very interesting justification for America not doing what she ought to do in science.

Yes, do you want to say something?

Dr. SESSLER. Well, that is no doubt true. But nevertheless, we have been saying that support of the National Science Foundation

is very important. And we know many of our colleagues who are experiencing very difficult times because of these circumstances.

The new science and technology centers that the National Science Foundation thought it was going to undertake, more than 300 institutions have applied with proposals, totalling billions of dollars. And none of those have been funded yet, as you know.

Senator DOMENICI. My point is that because of the structures that exist here you will find my friend, the Chairman, and I tonight, or tomorrow, will vote for a budget resolution that in its projection will double the National Science Foundation, will increase it this year 25 percent.

And as you smile and say, great, I will also predict for you that it has about as much chance of occurring six months from now when the appropriations bill gets voted out as the man in the moon.

Because there will not be enough money for space, veterans, and HUD, and therefore we will be lucky, in my opinion, to get a 5 percent increase instead of a 25 percent increase.

Now, having said that, let me ask the next question.

Senator FORD. That is the real world.

Senator DOMENICI. Which probably cries out for some restructuring across the board as to how we fund science and how we treat it in terms of prioritizing, because it is found all over the place.

It is not just in this subcommittee, it is over in another committee for space, it is in another committee for NSF, and the House has the same kind of problems.

Now, let me just proceed with three or four more questions and I will quit. If we are still number one, guardedly so, in science, then is it fair to say that it is not our pure science research that has caused us to slip in the international marketplace in terms of the production of goods and services and wealth that is competitive by the Japanese, Germans or others?

I would assume as scientists you would have to say that follows like day to night. If it is not that, and they are beating us in the marketplace, then it must be something else, other than science. Is that true?

Dr. BRINKMAN. Yes.

Senator DOMENICI. Okay. I frankly believe it is our lack of ability to apply science in the marketplace with the same rapidity and the same efficiency and effectiveness as they are doing, which leads itself to the area of engineering and industrial engineering and industrial management as a science.

And then frankly I want to tell you right now that I assume that is one of the most significant American shortcomings in the short-term, and short-term I am speaking 20 years, but we are making some headway in that regard.

Now, having said that, if you had been called here today to speak on the NASA program and a 25 percent annual increase in that budget in order to set our goals, and you knew that it too was going to come out of a shrinking budget, might I just ask for a quick answer?

Would you be as concerned about funding that, if as a matter of fact there was going to be less money for other aspects of science that you represent? Or would you be for NASA going up what the

President seeks, namely about a 19 to 25 percent increase, about $2.5 billion a year increase? Can you answer that?

Dr. SESSLER. Well, I have indicated in these graphs what I thought the priorities ought to be. And I guess I still stand with that.

These are physics, and it excludes space science, but it says that we must have an increase in small science as well as new construction. And if I understand your question correctly, if funding of NASA would not allow for an increase in funding of basic research in small science and physics, then I would be against it.

Dr. FISHER. The question is the question of the glamorous projects as against the projects that mean something.

The communications satellites are important for us. It is a real tragedy that this country has not been able to fly satellites. We are losing out there, seriously. And that affects both, if you like, big science, because they are expensive. But there is much small science that goes there. There is communications, there is the industrial things.

So that aspect of the NASA budget I would want to see. I would want to see it directed that way. But just a large increase so that frankly we can put a man somewhere else or a woman somewhere else or a schoolteacher—I hope to God never again—that I would be strongly oppose to.

Senator DOMENICI. I do not need everyone answering. I am just going to try to get through three or four just to show you the kind of problems we have got.

Dr. BRINKMAN. Well, I feel that what I have tried to say is that within basic science I believe the National Science Foundation and small science has got to be the highest priority item. And that includes the space science part of NASA.

Senator DOMENICI. All right. Well, let me then tell you for you who are advocates of the National Science Foundation, and I certainly am, that I believe it loses some of its strength in the United States Congress because of a real or perceived notion that it is kind of the rich get richer program. Those that have superior universities get all the money.

And frankly I think that, as peer reviewers, that has to be the case. I submit to you that if you want to broaden the support for the National Science Foundation you ought to help come up with some new ideas on peer review.

I know that sounds like something sacrilegious. But I truly believe we live in a political arena. And when you can map National Science Foundation funding, you really get an awful lot of Senators, and I do not know about Representatives but I assume, who will say, why should we fund it so heavily when we have a budding university system that cannot get in the door because it has not yet acquired one of these sophisticated scientists?

The uppity crowd that everybody will respond to their applications to fund them, and where do we grow? And I think until we find an answer to that, I suggest it is a very real one.

I am not sure that I have tried to be fair in my statements. I am not trying to be negative about it. But I think it is a real problem.

Now, let me move to my last question. If you were to assess, you have had me go through a list here, I could add another and I do

not want to bore you with it. But we recently have another major program that clearly the United States Government will do or it will not get done, the mapping and perhaps sequencing of the human genome.

If there is a project out there that clearly directs itself at potential wellness for humanity, and provides a research tool to the scientists studying illnesses, it is it.

It is a multi-billion dollar project. And I assume we would have asked the same question about it. Would you fund it if it was going to restrain some of our activities elsewhere?

I might even pit it up against the SSC and space, and then you would have a real interesting set of circumstances that we have to confront. You all are aware that it is a major new initiative. It finally has been put into quantity of dollars to be spent and what it might accomplish.

I myself think it is probably for humankind more important than the SSC. I believe it may make some breakthroughs in genetics that are incredible. And there we do take a chance that the Japanese, not now but within the next seven or eight years, will do it.

They are going to do it for a different reason. Because with that they may obtain the entire diagnostic and medical kind of marketplace that we have been superior in, including pharmaceutical products and the like. So those are the kind of problems that we have.

Is there any way that you can see, and this is my last question, that we could better use the resources of science that we have in this country, that we are either paying for, supporting, or others are supporting, or the private sector is supporting, is there any way short of new money, which I am all for in science, that any of you see as a way to better use what we have got?

Dr. FISHER. As you can tell from my accent, I was educated in Great Britain, and that has not been a rich country for a long time.

So at an early stage in my own research career, the question of thought versus dollars came up. And it was placed on me by my research supervisor, a canny Scotsman.

And he occasionally made the point, since we were often competing with the United States, that in their well-supplied laboratories they could do things rapidly, but they were not often forced to think of the other solutions.

I believe it comes down time and time again to allowing the brightest minds, the brightest young minds, wherever they come, and giving them sufficient, it is often not so much to get cracking and thinking. And I think they come in all parts of the country.

One of the upsetting things of the focusing of resources is just this point you made, Senator, that the resources tend to get put into the biggest places because they come in large chunks.

So I frankly think that although it is important to have big facilities, one has to look much more carefully at which ones we should have. And if we do not have some that are not in other parts of the world, we may still have and I think we still will have the best graduate training program and the best opportunities for young men and women.

When I see that some of our students are looking towards Europe in condensed matter physics for healthy careers, that the United States is no longer the first choice of foreign graduate students who come and train here, that is when I get concerned.

So time and time again I think it is the individual, the young person who will think. And if there is not enough money for the big projects, other ways of doing it cheaper will be found in time.

And if the only way we can do it with the largest amount, I would like to quote what Professor Anderson said at these hearings last year, this same hearing, if that is the only way we can go forward, are we certain that that branch of science has not reached its natural conclusion for the time being?

Senator DOMENICI. Well—yes?

Dr. SESSLER. I think, Senator, you raise a very important and interesting question about prioritization, and prioritization within science.

And I think it is fair to say, it is certainly my opinion, that this country, science in this country has suffered during this present Administration for the last eight years or so from lack of an adequate scientific advisory apparatus in the Federal Government, particularly for the President. And I would hope to see that changed in the future.

Senator DOMENICI. I would not disagree with you at all. In spite of increased funding, when you add defense to the other research, we have been on the upswing, yet we are suffering from lack of direction, it seems to me, and certainly the universities have a legitimate complaint with reference to resources.

My last observation to you all is that one thing that I think we ought to do is to find a better way to permit the three basic sources of science in this country, private sector, universities, and institutions that the Federal Government owns, I think we have to have a better way to synergize them.

I think we have compartmentalized them too much, and we are somewhat covetous of keeping them separate. And consistent with being principally an educational institution, I see no reason why universities cannot be more involved with the private sector, and why the national laboratories cannot be used more by private sector and universities in some way that captures more of their assets rather than leaves them sort of languishing within their assigned domain.

And I believe that is a rich area of potential increased activity that I hope one of these days we are going to work on.

Having said that, the SSC to me is typically an American approach. If we do not do it, it will be one of the few times when we have been presented with a project of this scope and have not done it.

I myself have always supported it. But to tell you the truth, I am profoundly confused at this point in terms of where I would set my priorities. And I am not so sure at this point that I would set that much there.

If I knew how much the foreign countries and the others would pay, I might be more disposed to move in that direction. And I may indeed, I might support it with $100 million instead of $300 million.

But frankly, I see some very big holes out there in science activity, and I am not sure this one is going to plug the hole or a series of holes that I see with as much urgency as some others.

Thank you very much, Mr. Chairman, and thanks to all of you.

Senator FORD. Thank you, Senator Domenici. You go over and work on the budget and get it increased 25 percent, and we will see if we cannot hold it.

Senator DOMENICI. We will.

Senator FORD. Two or three questions and I will not keep you all very long.

Dr. Lederman, you were kind of the only one that was in step when your partners were watching you march there a while ago, but yet you were out of step with the other five somewhat.

Would you expand on your position as it related to whether we are still number one, and you began to say the slippery slope was not particularly——

Dr. LEDERMAN. Thank you, I would be glad to. I think the thing I put more emphasis on, and it came up from time to time, is the perception of the scientist to be, the new principal and young investigator, the kids.

What is their perception of science? And that is why I put a greater emphasis, not on the absolute value, but on the slope, or as we say the derivative. Where are we going to be five years from now?

And that is where I think these young people have an enormous perceptive notion of where we are going, what is the dynamism of our science. And they can sense this long before us older people can sense this.

And therefore I put a much greater weight on their sense of where we are going. What are the budget fundings? And if a condensed matter young person looks at the field and says, shall I go into this, and look at these terrible budget cuts for my university, and I chance my mind, I predict then that that slope will be a depressing thing in all fields of science, as you have heard.

So, fundamentally, I put a bigger waiting factor than my colleagues do on a period of five years rather than on 1988.

Senator FORD. Let us take that a step further then, Dr. Brinkman. You made a survey which you alluded to, and it covered the average age of those in the field of education. Is that the way I understood your survey?

Dr. BRINKMAN. The average age of physics department.

Senator FORD. OK, physics department. That it was growing.

Did you check on the percentage of how many of those from other countries might be employed now in physics departments as it related to those from this country?

Dr. BRINKMAN. We did not check that. Of course, we looked into the question of how many graduate students were from foreign countries.

That, of course, has gotten very large. It is in the 40 some percent now. That has not dropped off at all. That is true across most of the fields of physical sciences and engineering today.

So the number of people who are being trained who are foreign students today is much, much larger in this country than it has ever been before. And at least my experience going around the

country is that the faculties of many universities are being filled with people from various foreign countries. And I think that is a good thing.

We are bringing bright people into the country, and we are making use of them. That is OK. So, but I do not have the number for what fraction that is.

Senator FORD. Let us look at Dr. Lederman's concern. I think the answer is that we are still number one, but how long will it last?

We are talking about, as it relates to uranium enrichment, a new process. We are five years ahead, you know, and somebody said, now maybe we are only two years ahead.

And we may be less than that, and some other country is going to come with that process and have it worked out before we do. Yes, Dr. Brinkman.

Dr. BRINKMAN. I was just going to make a comment on this, being ahead.

You know, one of the most interesting cases I see is a very explicit one, and that is in a thing called nuclear reactors for what is called neutron scattering for physics research.

And the Europeans have built a very large facility at Grenoble that is extremely well funded. And in fact very much better funded than ours, and our equipment is to a large extent out of date.

It is very interesting, however, that so far we have been very lucky. Because we in fact in many ways do just as well as they do in that kind of research, and it is because of the fact that they have of this ILL is funded by all the European countries, and they have to go through a program proposal sequence and things like that, and have a hard time getting sufficient time to do good research.

In this country we run our reactors less formally, and people when there is a good problem that should be worked on, it is worked on. And so we have managed to stay up, but, you know, if they ever change the rules over there, the things would change very quickly.

So people look at this and there is a very big discrepancy in the way things are being funded in those two places.

Senator FORD. Well, you may think we are better off, but we in Congress have 535 countries to deal with in order to try to work out a decision.

Dr. BRINKMAN. It does not work very well.

Senator FORD. Yes, at times it work very well, and I do not know whether that is luck or what.

Since I have been on this Committee I have had some interest in research and development, and one of the reasons I took this Committee is to see if there could not be something done to improve the direction, because I see us gradually falling behind in every field.

We have the attitude that when we invest, if not today, we want to collect tomorrow, and other countries, say it may be five years or 10 years, but they begin their process and they are not looking at that being done tomorrow.

I do not object to investing today and being a millionaire tomorrow attitude. But I do think that we have to understand it is going to take some time.

The accusation is that we have nickeled and dimed a lot of projects to death, and have never funded anything to completion. That if it is going to be a white elephant, hell, let it be a white elephant.

But pursue it, because you are always going to have frustrations because you cannot complete an idea and move on with it. Is there anything to that? Should we go on? Should we, instead of having it so thin that we never complete anything?

Everybody wants small science, they want to start this, that, and the other, and you do not want to hold them back. But is there any reason why we should not look at something, look at the scientific community and they say, this is what we should be doing?

Let us pursue it to completion, according to the trickle down theory, well I heard that a long time ago and I have never been trickled on much. But, is there any basis for using that sort of application? Does anybody want to hit on that a little bit?

Because this is a political problem. I mean, I understand your problem, you are frustrated with us. Senator Domenici and I are frustrated because we cannot do more. And I will get into that prospect in a minute.

Yes, Dr. Fisher?

Dr. FISHER. Well, I regret that what I feel is important is that when something is working well there is a temptation to say, well that could look after itself on, you know, a few percent less, and if you ask someone honestly they will say yes. A few percent less, go downwards.

It is the lack of glamour in supporting the things that are working well. And I would like to pick up the materials research laboratories which have been rather well spread across the country.

They are a form of funding which goes in in quite large blocks, but to a university, with the condition that it is used flexibly there. They have been reviewed, some of them have been closed down. Unfortunately, now they are just being cut back.

I think in honesty also, though, Senator, I must respond that not all branches of science go on fruitful forever. This is very painful for those that have spent their lifetime in it or even a fraction of their lifetime.

There is a question of a sunshine law for different areas of physics, for different areas of chemistry. Some communities manage that better, and when the investments are smaller again it is easier to drift.

So in honesty I think one has to look. But it is extraordinarily frustrating to set up a wonderful laboratory that is working well, and then starve it because it cannot pay its electricity bill. And that is where I think we are penalizing ourselves.

Senator FORD. Let us talk about the guts of what you all have been talking about a little bit today and that is the new students, the new bright ideas.

Let me ask this question to you. Is the SSC likely to increase the number of bright students choosing science and engineering, because of the glamour and high visibility of the project?

Or is the SSC likely to have the opposite effect by draining money from small science where most of the teaching of students is done?

Dr. QUIGG. I would like to respond to that, if I may.

Senator FORD. Dr. Quigg.

Dr. QUIGG. First of all, I would like to point out that when we define small science for the point of view of Mr. Brinkman's survey, many of the groups working in high energy physics in universities are small groups, a professor or two, a couple of post-doctoral fellows, three or four graduate students.

They share the same problems, the same frustrations with their colleagues in other sciences who do not work at central facilities. So when we talk about small sciences or small groups, it is important to understand that we are talking about, in general, all the university groups and not simply those that do not use shared facilities.

With respect to the inspirational value of the SSC, I think it certainly will stimulate young children to think of scientific careers, or at least to think of taking science courses.

I have had occasion in the last year and a half to travel extensively in this country, talking to people about the SSC. One of the great benefits that that has had for me as a personal experience is that, having gotten people's attention with the grandeur of the project and also we have to admit with its price tag, they do want to hear about the exploration, about the science and so on.

And it is quite typical when I give a public lecture in some small town to have 50 or 100 junior high school students show up with mommy and daddy in tow to learn about basic research in general and the SSC in particular.

If we use these large projects as ways to attract people's attention, the space telescope is another one. The space telescope has cost $2 billion, and it is something which is essential. It will expand our reach into the universe by a factor of 1,000.

If we can use these things not to drain away the resources from other things but to give a focus to science so that young children can see things to be excited about, they will then go on and learn about other subjects and take up scientific careers that need not be focused in these particular areas.

Senator FORD. Dr. Sessler?

Dr. SESSLER. Thank you.

Senator FORD. I woke you up a little bit.

Dr. SESSLER. I would say there is no question that the SSC will be intellectually attractive to students, and it will attract students and bring more people into science, and make this country foremost in that field of research. There is no question about that in my mind.

At the very end, though, you said, would this attract students away from other fields, small science? And that really bothers me.

Senator FORD. Or the reduction of funding, with funding going there and not——

Dr. SESSLER. I have testified that I do not think we should do that. And I say again very strongly that we should fund small science and in addition fund the SSC, and then we will attract new students into science and keep training people in the small science.

Senator FORD. We have done two things here in Congress. One, we have reduced the income to the Federal government. We have

increased defense and entitlements. And yet we are trying to balance the budget.

We are balancing the budget on the back of what has made this country great, in my opinion, the ability to have innovative ideas and pursue those. And a statement my father told me, he said, a long time ago, at 63 it is a long time ago.

But he said, son, in this country you have the right to go broke. And I did not understand that at that tender age. But I do now, because I had the right to pursue a dream. I am not sure that my grandchildren will have the same field of pursuit that I had. It has changed and it is different, you know.

When I first started growing up we would get 100 pounds of ice every other day, and the man would make some money by bringing it from town. And finally they dug the holes and put in the poles and brought electricity out there. And we started, like our boys and friends in town did, you know, it was quite an experience for me.

We always had something new and we could pursue it. I am very, very concerned about so much advancement and we are not able to tick it off and stay with it. And that is why I do not want to lose in what we are doing here the opportunity to continue to whet, as we would say, the young prospective physicist's, scientist's appetite to get into a field and to work.

And let me just pitch out one more item. If we should in our judgment, and I started to say wisdom but I am not sure Congress has much wisdom. It makes a judgment every so often, based on the wisdom we have been given.

What if in some of these fields where we make the grants, loans, et cetera, that if they pursue these fields that we forgive them of their indebtedness? Do you think that would have any results, would that jar anything?

Say, in your physics field or whatever it might be, and they pursue it to their doctoral level, and then they go out into the world, and they owe us money, from the grant or loan to pursue their education.

If they do that, we as a government will say, if you pursue that field, at some point we will be in the posture, or in the position of forgiving you. We would have to work out the regulations or the rules, and Dr. Brinkman you——

Dr. BRINKMAN. Well, I guess my general impression is that that is not a big effect.

Senator FORD. That is not being effective.

Dr. BRINKMAN. A big effect.

Senator FORD. Oh.

Dr. BRINKMAN. In the sense that most graduate students in sciences are funded by some means or another, so that they are not spending their own money and going into enormous debts.

Senator FORD. But would they not then be obligated to AT&T? If AT&T is funding them? Or——

Dr. BRINKMAN. No, I want to be very explicit. AT&T has a fellowship program, a number of fellowship programs, a fairly large set of programs.

Senator FORD. Yes, I understand that.

Dr. BRINKMAN. And we do not make any of our people that we give fellowships to obligated to us in any way.

Dr. FISHER. I hate to interrupt you, Bill, but I understand, Mr. Chairman, that you are also referring to the undergraduates.

Senator FORD. That is correct, yes.

Dr. FISHER. I know from personal cases, you are addressing the graduate students, and I know from a personal case very close to me that paying off that undergraduate debt is something that hangs over the individual's head.

Senator FORD. And prevents the pursuit, sometimes.

Dr. FISHER. Well, is a discouragement. And if you look at the discouragement at the early stages, especially if we look at bringing minorities and women in, then that seems to me it gets to be a very big discouragement.

I think what I agree with Dr. Brinkman is at the stage we get to graduate school, that is not the problem. But we have to be worried for our future in this country at the high schools and at the undergraduate levels, and feeding them into the areas that are important to us and giving the people the opportunity.

That is where I see the problem. Most of us, I think, look at it, you know, they are there as graduate students, where do they go with their careers then? But I understood you to be addressing the earlier stage.

Senator FORD. It is just like the professor told me when I started, he said, you will not like me at all when you leave this institution. But he said, by damn, there may be the answer to cancer in this room and I am going to try to drag it out of you.

And he worked awful hard at it. Of course, he made a politician out of me, he drove me from that field. And you can blame it all on him.

Gentlemen, there may be an effort, not in this Congress but in the 101st Congress, to look at the total scientific investment, I am talking about the whole field now, of the Federal government, and it could be focused in this small subcommittee.

If you have any thoughts, if you have any druthers, if you have any direction for those of us that are honestly, sincerely trying to reach a point where we can see some improvement and generate some thought and some interest through what we can do, and when Senator Domenici talked about almost 23,000 in the Department of Energy, you begin to look at how big we are.

And are we so big that we have lost the basic thoughts? Are we so limited to categories that we will not get out of it? There is no fluid ability? We need some advice and counsel from some of the best minds, and I am one who likes to see it spread out rather than limited to the Cornells of this world, as Dr. Fisher said it.

Of course he said, when he went there it was not one of the better ones, but after he got there it became one of them. And so, Dr. Fisher, I do not mind if a man tooteth his own horn. I have been doing that for a long time. People have agreed with me so far. Only I need 51 percent, you may not need quite that large a percentage.

So I want you to think about that, because I believe, I honestly believe that it is coming. A procedure as it relates to encourage-

ment in undergraduate and graduate work in all these fields is something that will come maybe this Congress.

Because we are honestly looking at it right now, to whet the appetite and to help those who have the better and brighter opportunities to move forward, and I think it requires reorganization of the procedure this government uses.

Because we find ourselves not only spreading things too thin in various and sundry departments, you know, in this area, we find it in others. And once you go too far in one place you find that the gap is reduced on the other side. And so it is like the water in the bucket, however it is tilted that is the way it levels out.

Gentlemen, I thank you. You have been most kind. Does anybody want to wind up here?

Dr. BRINKMAN. Well, you were commenting on planning and introducing young people into the field. And one of the things that I, one of my issues on that is concentration of fellowship awards on the students rather than on the university faculty.

And I think that if you want to do more in that direction, getting recognition for achievement as early as possible for students that do things in science is very, very important.

Senator FORD. What is early as possible, Dr. Brinkman?

Dr. BRINKMAN. Even in the undergraduate days.

Senator FORD. Dr. Quigg was talking about seniors and juniors in high school.

Dr. QUIGG. Or even junior high school students.

Senator FORD. Even junior high school. I have a granddaughter who is 12, and she amazes me with what she can do. Had a computer in the second grade.

And I have said that more than once, but you know, when you are in the second grade and you have a computer that you share with another student, and then she gets one at home, things are beginning to progress pretty rapidly for this old man.

Dr. FISHER. I certainly second the fellowships and scholarships for young people. It makes them feel and their parents feel and society feel that these are the important goals of a country, and you give it to the individual.

And, Mr. Chairman, I would like to thank you for your patience in listening to people tell you a lot of things that you clearly knew very well and understood already.

Senator FORD. Well, you know, I found out that I would rather hear it twice than not at all. Thank you all very, very much.

[Whereupon, at 11:40 a.m., the hearing was adjourned.]

APPENDIX

ADDITIONAL MATERIAL SUBMITTED FOR THE RECORD

The American Society of Mechanical Engineers

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Statement on the

U.S. Department of Energy

FY 1989 Budget Request

As it relates to

Funded University Research

Submitted to

Subcommittee on Energy, Research and Development

Senate Committee on Energy and Natural Resources

U.S. Senate

Prepared by

A Task Force of the Board
on Basic Engineering

The American Society of
Mechanical Engineers

April 14, 1988

Mr. Chairman and Members of the Committee:

We are pleased to have the opportunity to address the 1989 budget of the U.S. Department of Energy, with particular attention paid to its effect on engineering research carried on in U.S. universities. This statement has been prepared by a task force of the Board on Basic Engineering of the American Society of Mechanical Engineers (ASME). Our society, which celebrated its centennial year in 1980, has a current membership of 118,000, including 22,000 students. Our statement represents the opinion of our task force and the leadership of the Board; rather than an official position of ASME.

The subject of energy in its broadest connotation is a central focus of the mechanical engineering profession. ASME, as the principal technical society of the profession, is, therefore, very interested in and generally supportive of the U.S. Department of Energy budget; however, we here are focusing on those positions of the FY 1989 budget which relate directly to energy-related research at our universities, both public and private.

The term "engineering research" is not well understood or agreed upon by most non-engineers. Research, in general, is concerned with new knowledge and understanding of our world. Engineers engage in research activities covering the broad spectrum from fundamental studies to those which are applied in nature. We use

the terms "fundamental" and "basic" to denote research which has as its objective the determination of new insights into our physical world. Such endeavors are usually long-term, perhaps lasting a decade or more, providing the bases for more applied, developmental studies at some indeterminate later time. "Applied" research, on the other hand, generally deals with a specific process, device, or system and is most often of a shorter duration - on the order of months or a year or two. Goals of such endeavors are generally useful products or processes, ready for transfer to industry or the public.

The Department of Energy has programs which would fit the categories of both "basic" and "applied." While funding is provided to universities under most DOE programs, the applied areas, which often involve relatively large-scale facilities and have a short duration, are generally pursued at the DOE laboratories. Universities are much better prepared to undertake long-range, fundamental studies and find it difficult to develop facilities to address large projects in a short time-frame.

We must also remember that research activities at universities have a dual purpose. In addition to developing new insights and understanding of our world, such activities also provide an essential part of the education of advanced-degree students. They are, hopefully, among the best and brightest of the current generation. These graduates will be the professors, laboratory

investigators, and industrial research directors of the future, and often the directors of engineering and chief executive officers of our technically-oriented companies. This educational component of university-conducted research obviously cannot be neglected. It is vital that we continue to educate enough advanced-degree engineers, and that we attract our best U. S. students into such programs, so that we will remain at the forefront of technology and continue to excel in an increasingly competetive world.

While the Department of Energy is not the only federal source for funding university research, it represents a significant component of the U. S. government which does provide such support. It is also important, in our opinion, that there be an underlying motivation for research endeavors. The area of energy is of great interest to mechanical engineers, as was stated earlier, and the support of basic energy-related research within an agency that is also concerned with practical, applied energy issues has obvious benefits. The results of basic research, motivated by energy concerns, will be picked up and used sooner and more effectively when both are administered within the DOE.

A final general comment before we address some details of the budget document: University research is absolutely dependent on federal funding. Some have suggested that financial support for university research activities should come, in large part, from

the private sector. In recent years, this has occurred to a small degree but nowhere near the level necessary. Industry is both competetive and concerned with its financial state. For these reasons, few companies are able to support basic research, with uncertain outcomes, that may not even be achieved for a decade or more; thus, their interest is, understandably, in more applied areas. In these cases, there are very few firms willing to support research, the outcomes of which will be available to their competitors - both domestic and overseas, as well as to themselves. Thus, the federal government must be the principal provider of research funding at universities. This responsibility has been realized for many years; we wish to emphasize the importance of such a federal role. The dual benefits of research accomplishments and graduate education are dependent upon continued support of this nature.

We now address a few specific details regarding the FY 1989 DOE budget; specifically, those programs which provide funding for research at universities.

Basic Energy Sciences

This is the principal Division of the DOE which addresses fundamental, long-term research issues. It is, thus, the source of a major portion of university funding within the DOE framework. BES is a division of the Office of Energy Research.

Of its FY 1989 budget request of $375,662,000, the sum of $344,452,000 will be available for support of research operating expenses. There are six program areas in this budget, the largest being Materials Science, which accounts for over $134 million, or approximately 38% of the total. Other divisions and their portions of the budget are Chemical Sciences 27%, Applied Mathematical Sciences 12%, Advanced Energy Projects 4%, Energy Biosciences 6%, and Engineering and Geosciences 9.7%. The last figure is a bit misleading. The combination of Engineering and Geoscience is an administrative convenience; the two programs are separately budgeted and have their own technical staffs. The funding level for the Engineering Research Program is $15,443,000, or about 4.5% of the BES figure.

In the overview statement for the BES budget, it is noted that "more than one-third of BES funding supports university-based research." The national laboratories receive the largest portion of the total budget. We have no quarrel with this; however, we are concerned that ongoing commitments to large national laboratory facilities and large complements of technical and supporting personnel make the remaining funds the first to be considered for cuts when budget pressures are encountered. In the current situation, when major initiatives include the superconducting supercollider and high temperature superconducting materials, we hope and trust that these important and very costly ventures not be undertaken at the expense of

basic research programs carried on at universities.

The Engineering Research Program, within BES, was started in 1979 with the realization that engineering research is both complementary and an important corollary of the traditional scientific thrust of the Division. We suggest that engineering research undergirds all of science research in that it provides the link between new scientific knowledge and those products and processes which make use of such discoveries. Without this important link, there would be significant delays in the transfer of scientific knowledge to practicality. At its present funding level, we believe this BES program to have insufficient resources to reach its potential. It is also important to note that over fifty percent of Engineering Research funds support university projects.

Some important current thrust areas of the Engineering Research Program include intelligent machines, the work being done at Oak Ridge National Laboratory; and a collaborative program involving the Idaho National Engineering Laboratory and M.I.T., Stanford University, and the University of Minnesota, concerned with materials processing, automated welding, and fracture characterization and prediction in structural materials. These two relatively large programs are obviously important for improving manufacturing processes.

Another new thrust is envisioned in the area of multiphase flow. Two large ventures in this area are being proposed, one at Rensselaer Polytechnic Institute, the other a collaborative effort involving Argonne National Laboratory and MUEC, a consortium of universities in the midwestern United States. This important research in multi-phase flow has broad application in such energy-related areas as coal combustion, transport of coal and other solids through pipes, and nuclear and fossil power plant safety. Research in multiphase flow - that involving combinations of solid, liquid, and gaseous media - is receiving very little attention by any other federal funding agency. Without substantial increases in the program budget, these efforts can be initiated at only a very modest scale, if at all.

Other research areas are discussed in the program summary of the budget document. At its current modest funding level, this program has an excellent reputation for both quality and the breadth of subjects being supported. We suggest that the potential of engineering research is many times that which is being supported at present.

University Research Support

The University Research Support (URS) program is one means used to address those concerns discussed earlier regarding the importance of engineering research activities carried on at

universities regarding both technology transfer and manpower development. This program has continually supported activities related to national laboratory-university collaboration, emphasizing the use of unique laboratory facilities by professors and students, and a number of direct stipends to students as a means of encouraging their pursuit of advanced-degree education.

The URS program presently supports collaborative efforts, primarily in pure science. We are hopeful that, in time, engineering research will also be carried on under this program at a significant level.

We are pleased to note the FY 1989 budget request of $15,770,000 for this program. It provides a valuable source of support for interaction between university personnel and the laboratories in ways that would not be possible otherwise.

University Research Instrumentation

This is another very valuable program of the DOE. The University Research Instrumentation (URI) program allows university researchers to acquire state-of-the-art instrumentation to pursue energy-related projects that would not be possible without such equipment. It also provides valuable training for the students involved in these research efforts.

Major recent advances have been made in instrumentation, particularly electronic and optical devices. Effective use of such equipment can allow research of entirely new types, as well as enhance productivity of established research thrusts, by orders of magnitude. With state-of-the-art equipment, certain research previously thought to be impossible can now be pursued.

Our only concern with this program is its budget level. The $5 million request for FY 1989 is the same as in the previous two fiscal years. We believe this program to be one of the best means of developing universities' research capabilities and encourage regular and substantial budget increases beyond the coming fiscal year. As with other DOE support, this program is currently supporting research instrumentation principally in pure science areas, with engineering receiving a relatively small percentage of the funding. We anticipate that increasingly larger fractions of funding in this program will be directed toward engineering research in the future. The importance of this program cannot be stated too strongly.

Concluding Comments

We are pleased that the DOE budget for the Basic Energy Sciences has shown regular increases during recent years and that the area of Engineering Research, established ten years ago as a separate program, has shared in that growth. We encourage your subcommittee, the House Science and Space Technology Committee, and the Congress, as a whole, to continue this commitment to fundamental research under Department of Energy sponsorship. We hope that, in addition, there will be increased awareness of the pervasive nature of Engineering Research across BES and the other areas of DOE. While the Engineering Research Program has grown, its relative standing of 4.5% of the BES budget is still far below the level appropriate for its potential contribution. Certain of the new initiatives deserve your consideration; we especially encourage support in the area of multiphase flow.

To put this matter into perspective, it is noteworthy that the FY 1989 budget request for the engineering portion of the National Science Foundation includes support at a level representing approximately 12% of the total budget for that agency - which we believe to be too small. When one considers relative numbers of faculty, students, and subsequent practitioners, it seems clear that engineering research should be funded at appreciably higher levels relative to science. Again we remind you that the funding

for Engineering Research in BES is approximately 4.5% of the total budget for that portion of the Department of Energy. We believe this figure to be much too small and not at all representative of the relative importance of generic engineering research.

Basic research cannot be turned off and then restarted at a later time without serious disruption in both a line of investigation and in manpower education. Continued and increased support in those areas which fund engineering research at universities is vital, in our view.

We are also very supportive of the University Research Support and University Research Instrumentation programs. These two, particularly the latter, are forward-looking programs that will continue to pay dividends for many years.

While it may seem that we, like others who come before your committee, are saying "spend more money on the cause we espouse," it is our conviction that few other enterprises are as important in as many ways as are the research activities carried on in engineering colleges at our universities. Increased funding of programs which support this enterprise represent a sound investment policy which will be returned many times over.

Task Force on the

U. S. Department of Energy, 1989 Budget
As Related to Funding of University Research

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TESTIMONY OF

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MARTIN MARIETTA ASTRONAUTICS GROUP

PARSONS BRINCKERHOFF

BEFORE THE

COMMITTEE ON ENERGY AND NATURAL RESOURCES

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

UNITED STATES SENATE

ON THE

FY'89 BUDGET REQUEST FOR THE SUPERCONDUCTING SUPER COLLIDER

APRIL 12, 1988

We are pleased to be given the opportunity to submit this written testimony to the Senate Subcommittee on Energy Research and Development. We are all members of the SSC Industry Group - over 50 commercial organizations who strongly support Federal funding of the Superconducting Super Collider (SSC).

Today, the United States is on the verge of leapfrogging much of what the world knows -- and the uses to which knowledge can be put -- through scientific achievement. We are entering the age of the Superconducting Super Collider.

We are committed to the SSC for several reasons.

First, there is the principle of scientific discovery. No single previous effort holds as much potential for uncovering answers to the fundamental nature and workings of our world as does the SSC.

Second, there is the principle of applied learning. From the construction and operation of the SSC itself -- to the expanded knowledge it is sure to yield -- we will have access to new tools for research and development in other fields.

Third, there is the principle of competitiveness. The SSC will enable the United States to assert new leadership in the world scientific community. Not only will the SSC be a respected international resource, but a seedbed for American technological innovation for generations to come.

Fourth, there is the principle of collaboration. Few projects have the potential to influence the long-term future of America's "know-how" and "can do" as does the SSC. Its benefits to education, to industry and to the government -- and ultimately to the American people -- seem almost limitless. Yet, no project of this magnitude could be undertaken by the private sector alone. Like the herculean efforts waged to unlock the mystery of nuclear fission -- or to place a person on the moon -- the SSC cannot become a reality without strong leadership by our government.

Fifth, and finally, there is the principle of the future. This nation has always been challenged by the unknown -- and has seldom shied from the pioneering spirit. We have always had a sense that our destiny lies on a distant horizon.

Science and society are indispensible partners in the quest for answers that make better lives and stronger nations. Technology is not an end in itself, but a means for enhancing life. Nor is knowledge its own excuse for being. It must always be a companion to intelligent action.

The time is fast approaching when a decision to construct the SSC must be made. Action is necessary to ensure that the resources for its full development will become available. We urge this Subcommittee and the Congress to be bold in its hour of decision.

While nothing is assured, we must be persistent in pursuing our objective. If we do not build the SSC, we will lose more than our competitiveness in high energy physics to other nations. We will slow our quest for knowledge and slacken the pace of American innovation.

We must again point to the future. The impact of the SSC will transcend the managements of our companies and the records of many Congresses to come. It is a bridge to the next century and a world in which today's young scientists will harness its knowledge and harvest its benefits. The SSC is <u>really</u> about the future, and we must rise to the challenge of technology leadership.

Supercollider R&D

March 1988

Universities Research Association, Inc.
Washington, D.C.

A cable-winding machine used for the fabrication of superconducting cables. A more detailed explanation of cable fabrication is given on page 16. (Courtesy of Lawrence Berkeley Laboratory)

The SSC R&D program is operated for the U. S. Department of Energy by Universities Research Association

The R&D Program

The Superconducting Super Collider (SSC) is an instrument for scientific exploration of the world of subatomic particles. The most important part of the SSC is an underground ring, 53 miles around, in which two beams of protons orbit in opposite directions at nearly the speed of light. At certain points, protons collide head-on with an energy of 20 trillion electron volts each, producing a shower of particles that physicists will study for clues to the basic structures of matter and the forces that govern the universe.

The successful development of superconducting accelerator magnets for Fermilab's Tevatron, where protons have been boosted to nearly one trillion electron volts, put the SSC within our technological grasp. The design of many components is based on improvements to existing equipment, appropriate to the the size and energy of the SSC, and satisfying the need for improved cost-effectiveness.

1988 marks the fifth year of detailed research-and-development work on the SSC. The Department of Energy allocated funds for preliminary research and development in the fall of 1983. In the spring of 1984, more than 150 participants from national laboratories, universities, and commercial firms studied the feasibility and cost of achieving the needed beam parameters. Several thousand pages of text and cost estimates were distilled into a summary, the Reference Designs Study, that formed the basis for continued discussion. Then, the Department of Energy contracted in early 1984 with the Universities Research Association (a consortium of 66 leading research universities) to direct and coordinate the SSC research and development program.

For this purpose the SSC Central Design Group was established, accepted the hospitality of the Lawrence Berkeley Laboratory, and began work on a non-site-specific conceptual design. Participants included Brookhaven National Laboratory, the Fermi National Accelerator Laboratory (Fermilab), Lawrence Berkeley Laboratory, the Texas Accelerator Center, and experts from many laboratories and universities in Canada, Western Europe, Japan, and the United States. American industrial firms also played an important role.

An early goal was to determine the basic dipole magnet style. In September 1985, after several different concepts had been developed and studied, a high-field superconducting magnet in which the field is defined by the placement of the superconductor was adopted as the basis for the conceptual design.

Simultaneously, all accelerator and conventional systems (other than detectors) were defined and assessed. The completed Conceptual Design Report details the scientific, technical, and cost aspects of the project. Since its publication in March 1986, the Conceptual Design Report has been extensively reviewed by independent experts.

President Reagan endorsed the SSC as a national goal in January 1987. The Department of Energy began the process of selecting a location for the SSC laboratory in April of that year. In January 1988, with the advice of a site evaluation committee assembled by the National Academies of Sciences and Engineering, the Department of Energy announced a Best Qualified List of seven sites, selected from 35 technically responsive proposals in 25 states.

For the past two years, the Central Design Group has coordinated a multi-fronted assault on the challenge of refining a technically assured, economically optimal conceptual design. A team of scientists, engineers, and technicians across the country has been engaged in bringing the idea of the SSC to reality. These have been exciting times for all involved, and we are pleased to offer this report on our progress.

Director, SSC Central Design Group

Introduction: Why Build the SSC?

The ultimate secrets of matter and energy are to be found in the world of fundamental particles. These particles compose all matter, and the laws they obey apparently apply at any time and place in the universe. The goal of high-energy physics is to map out this world and to discover the rules that govern its behavior. This knowledge has far-reaching consequences for our understanding of what the universe is and how it came to be.

Thanks to a host of new experimental findings made accessible by particle accelerators in the United States and elsewhere in the world, the last two decades have brought a radically new and simple picture of Nature on the most basic level. We have learned that all matter in its diverse forms is assembled from a few basic building blocks, called quarks and leptons. All known natural phenomena can be described in terms of a few fundamental forces acting among the quarks and leptons. The resulting synthesis is known as the Standard Model of Elementary Particle Physics.

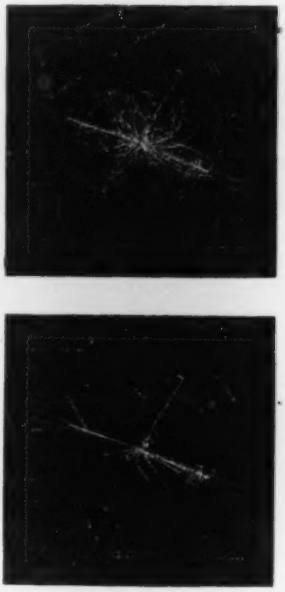
The Standard Model summarizes the remarkable discoveries made in high-energy physics during the past two decades and is consistent with everyday experience and all laboratory observations. It not only provides a framework for describing and understanding the world around us, but also provides important insights into the first instants after the creation of the universe. The Standard Model suggests new lines of theoretical research, hinting at a unification of the strong, weak, and electromagnetic interactions and at the possibility of incorporating gravity into quantum theory.

However, when extrapolated to much higher energies than we have been able to explore, conditions even closer to the moment of creation, the Standard Model is logically incomplete and inconsistent. It raises questions that can only be answered by new experiments leading toward a more fundamental theory of the subatomic world.

The instrument of choice for advancing these frontiers is the Superconducting Super Collider—the SSC. For more than half a century, physicists have been exploring the subatomic world with particle accelerators. To zoom in on this realm with increasing detail requires increasingly high energies. By the mid-1990s, physicists hope to use an accelerator—the SSC—twenty times more powerful than exists today.

By boosting two beams of protons almost to the speed of light and directing them into head-on collision, the SSC will create a shower of particles from the energy in the collisions. Particles will be created that have never before been seen. Sensitive instruments will detect and record the products of the collisions, which will be studied by international scientific collaborations.

Although many of the phenomena that will be revealed do not occur at the relatively low energies of ordinary conditions on earth, they play a crucial role in the unification of forces and in the dynamics of the early universe. By helping us to explore and codify the basic laws of nature, the SSC offers a deeper understanding of the universe and our place in it, and the possibility of putting that knowledge to humane and productive use.



Computer simulations of 40-TeV proton-proton collisions at the SSC. Colored lines represent the trajectories of particles produced in the collisions. Experimental apparatus at the SSC will be designed to measure the energies and directions of the particles produced in the collisions. Using a variety of computer technologies to select, record, analyze, and interpret data, physicists will probe the constituents of the proton in their quest for new understanding of nature's fundamental particles and forces. (Courtesy of Alan Weinstein, University of California, Santa Cruz)

How Does the SSC Work?

The SSC is a chain of accelerators, linked to boost barely moving protons to nearly the speed of light. The particles reach increasingly high energies in a cascade of four accelerators that make up the injector, from which they are fed into the collider ring.

The collider ring is the heart of the SSC. In this underground oval tunnel, 10 feet in diameter and 53 miles in circumference, two beams of protons are accelerated in opposite directions. The protons travel in compact bunches, with billions of protons in each bunch. Each second, the fifteen thousand bunches that fill each beam pipe make three thousand laps around the ring. On every lap, a pulse of radio waves boosts the energy of each bunch, until they reach their final energy—twenty trillion electron volts. The circulating beams are maintained at that energy and focused into head-on collisions where detectors can record particles produced from the energy of the collision.

Although an accelerator is simple in concept, the SSC demands that many complex systems function in precise harmony to actually achieve and record the proton-proton collisions.

Magnets and Cryogenics

Superconducting electromagnets in the collider ring guide each proton beam through a stainless steel pipe about one and a quarter inches in diameter. Magnets curve the paths of the beams, making it possible to build the accelerator in a loop, rather than a straight line, and thus realize substantial economies by repeatedly using the same components to accelerate and manipulate the beam. In the collider ring of the SSC, for example, the same accelerating station gives the beam a push three thousand times each second.

Superconducting magnets are used because they make possible substantial savings. At low temperatures, certain materials become superconductors, in which electricity is able to flow without resistance. Such a material, an alloy of niobium and titanium, will be used to fabricate the coils for the SSC magnets. The magnets will be operated at the temperature of liquid helium, about 4 degrees above absolute zero. Once these magnets are turned on, electric power is only required to drive the refrigerators that keep them cold. This is less than one percent of what would be needed to power conventional electromagnets built from copper cable.

In addition, superconducting magnets can create magnetic fields several times stronger than conventional magnets, thus confining the protons to a tighter orbit and reducing the ring's perimeter.

An important benefit of the cryogenic system is its contribution to maintaining the vacuum in the beam pipe. If the protons bumped into air molecules as they traveled around the ring, they would lose energy and be deflected from their proper orbit. The liquid helium for the superconducting magnets bathes the outside walls of the beam pipe and lowers its temperature. Any stray molecules inside the pipe tend to adhere on the cold inside walls of the beam pipe, out of the protons' way, just as a damp finger sticks to an ice cube.

Detectors

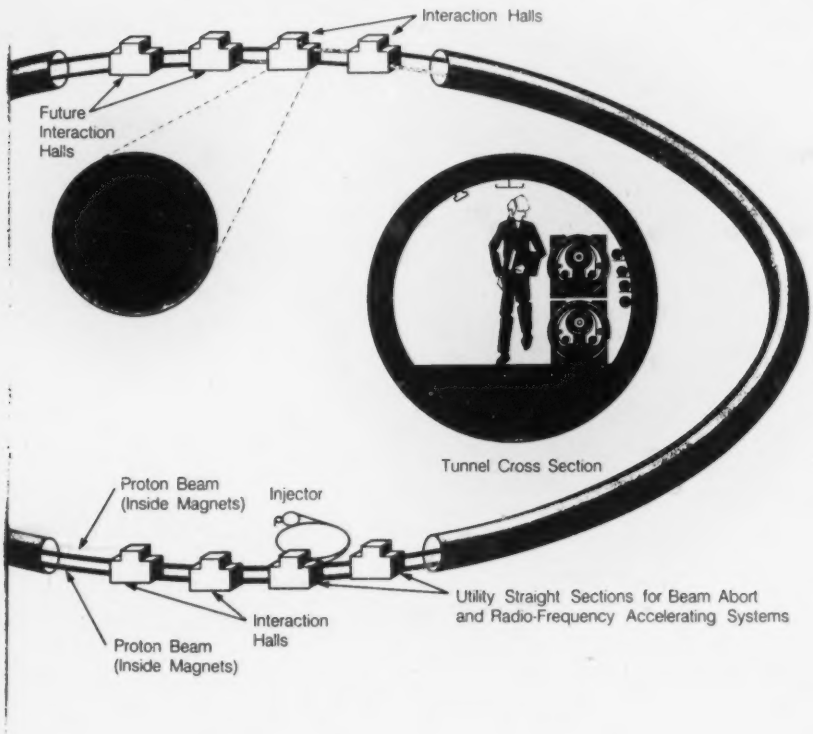
At the interaction regions the two beams are steered into head-on collision. Experimental apparatus at

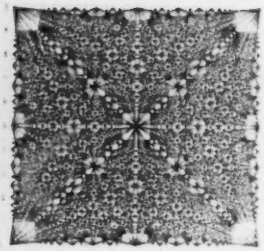
Proposed layout for the SSC. Interaction halls cluster on the two gently curved sides of the collider ring. In this perspective, the near cluster incorporates the injector complex, the radio-frequency accelerating system, beam absorbers, and two of the six interaction halls where stored proton beams will collide head-on. The far cluster adds four more interaction halls, two of which are reserved for development after research begins. The schematic enlargement of an interaction hall shows a detector surrounding the point at which two beams collide; the cross section to the right shows the position of the two superconducting magnet rings in the tunnel.

These points is designed to provide two kinds of information: to identify the particles produced in the collisions by measuring their properties, and to determine as precisely as possible the path each particle followed. Although similar in concept to existing detectors, SSC detectors will be physically larger and more complex. Using a vast array of state-of-the-art components, international teams of physicists and engineers will design, construct, install, and operate the experimental apparatus at the SSC.

Controls

Precise control is necessary to coordinate the five accelerators that make up the SSC chain, and to ensure that the magnets, power supplies, refrigerators, and their controlling computers function synchronously. Special instruments monitor these systems and relay their observations electronically to a distributed complex of computers.





Just as a child on a swing arcs higher and higher if he is pushed in time with his motion, a particle that executes an integer number of transverse oscillations on one or a few turns around the synchrotron will have its displacement from the central orbit significantly amplified by small imperfections in field quality, by joints in the beam pipe, or by small misalignments. This plot shows the locations of resonances—the operating points to be avoided—through tenth order in the horizontal and vertical tune plane

The Accelerator

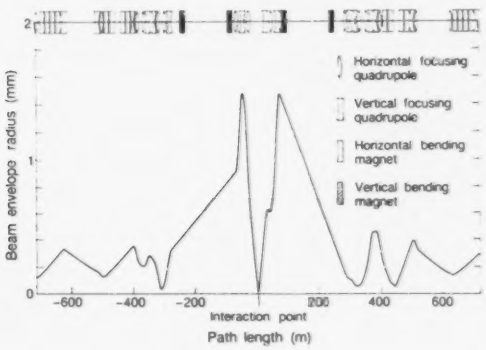
Accelerator designers associated with the SSC have had several important tasks: refinement of the design of the accelerator itself, the detailed specification of performance requirements for technical components, and the development of algorithms to monitor and control the performance of the accelerator complex. Some of the highlights of this work are described here.

Physicists in the Central Design Group have developed sophisticated computer algorithms to model beam behavior under varied conditions.

All the way around the 53-mile-long tunnel, the SSC beams must be accurately confined to the 3.3-centimeter-internal-diameter beam pipes. The magnets must be carefully laid out to achieve this with optimum beam quality and cost. A pattern has now been designed for the 2100 magnets in each of the two large, semicircular regions of each ring (the arcs) that confines the beam into a cylinder 0.068 cm in diameter, well within the aperture of the beam pipe.

More sophisticated optics are required to focus the beams into even tighter bunches at the collision points. To produce the desired high collision rate, beams at the collision points are compressed into cylinders thinner than a human hair. A special modular design has been devised that not only squeezes the diameter of the beams but is also extremely versatile, allowing a wide range of collision conditions appropriate for different experiments to be produced.

An important design decision for a circular accelerator is the size of the magnet aperture through which the beam travels. Although the cost of building magnets increases with the diameter of the required uniform magnetic field, field quality, which determines beam stability, becomes more difficult to attain as the aperture is decreased. Accelerator physicists at the Central Design Group first developed detailed theoretical tools—both numerical and analytical—to examine a range of possible apertures. They then



The modular structure of the SSC's final focus optical design is capable of focusing both beams to a diameter of 10 micrometers at the collision points. It is also versatile enough to meet a wide range of high-energy physics needs. Focusing and defocusing quadrupoles are represented here by focusing and defocusing optical lenses. The dipole magnets that bend the beam horizontally and vertically are also shown. The beam size at the collision point is 30 times smaller than in the arcs.

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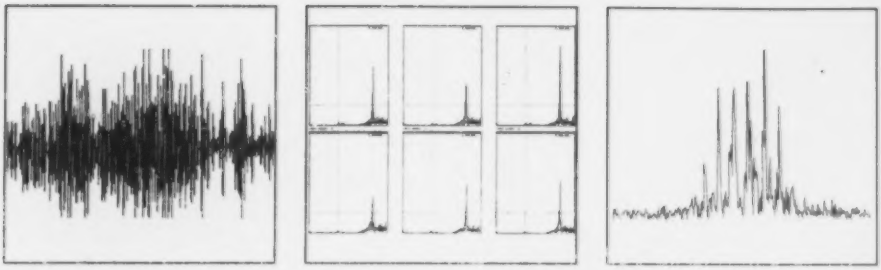


A strong online analysis capability available during the aperture experiment is important for efficient performance. Three windows visible simultaneously on one workstation screen, show (left to right) time variation of the transverse position of the beam, zoomed in to show individual turns; a sequence of the tune spectra, showing frequency components of the transverse oscillations, taken at uniform intervals during acceleration of the Tevatron beam; and one of the tune spectra zoomed in to show side-band structure.

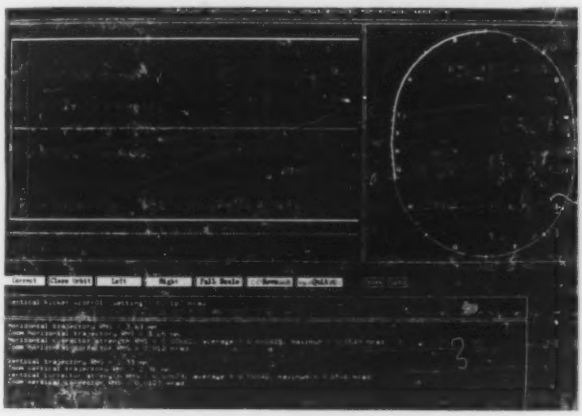
tentatively chose a beam pipe with an interior diameter of 3.3 cm as the most cost-effective for the SSC.

To check the predictive power and accuracy of the theoretical calculations, and to establish criteria for evaluating the SSC aperture, a team of accelerator physicists from the Central Design Group, CERN, Cornell University, Fermilab, and SLAC has been conducting a series of experiments at the Fermilab Tevatron. In a typical aperture experiment, the circulating beam is given a sudden kick that causes it to oscillate about its equilibrium orbit, while special sextupole magnets of known strength are powered to mimic the SSC environment. A pair of beam position monitors registers turn-by-turn beam position data after the fast kick for up to a few hundred thousand turns.

When the experiments are completed, we will be able to confidently set the magnet aperture. The knowledge gained in accelerator physics, particularly nonlinear dynamics, should prove valuable for all circular accelerators and for other particle beam devices.

Understanding of the behavior of a circulating beam has gained considerably from the Central Design Group's computer simulation of the operating SSC. The simulation runs on a cluster of scientific workstations, using interactive graphics software developed by the CDG.

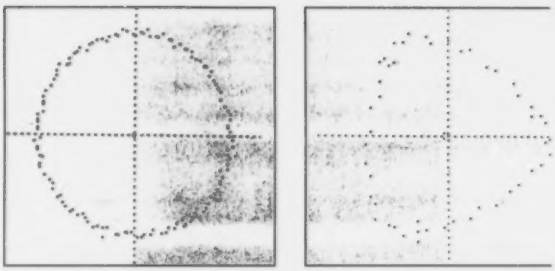
For example, the program allows physicists to simulate the process of first-turn orbit correction. During start up, the very weak beam that is first injected into an accelerator will inevitably hit the beam pipe before it completes its orbit, due to small magnet errors and misalignments. To correct the orbit, the location of the injected beam is reported by beam position monitors, and correction magnets in the first section of the machine are set to compensate for



Computer simulation of first-turn orbit correction in the SSC. On the top right of the screen, the operator sees a bird's eye view of the main SSC ring. Here, the beam orbit has been corrected for the highlighted section. The top left window indicates the beam pipe aperture with horizontal white lines. The colored horizontal and vertical beam position monitor readings show the corrected portion of the beam, on the left, and the uncorrected portion that hits the beam pipe, on the right. Various statistics used for beam diagnostics appear in the lower window.

steering errors. This procedure is repeated until the beam follows the desired path all around the ring. By simulating this procedure now, the beam position monitors and orbit correctors can be optimally placed in the actual machine.

The SSC operations simulation program will be useful in studying a broad range of accelerator operations. It will yield design specifications on various beam monitoring and control systems, and it will also define optimal procedures for various beam manipulation operations.



Data from an experiment at the Tevatron to help determine the most cost-effective magnet aperture for the SSC. Each dot in the phase-space plot represents the beam position data for one turn. (Left) When sextupole magnets are turned off, the phase-space plot shows beam behavior in a magnetic field of good quality. (Right) When sextupoles are turned on, the behavior of the beam in a less uniform field is recorded, and the phase-space plot is distorted; here it becomes triangular. The shape and amount distortion provide information about the response of the beam to variations in the magnetic field quality.

Accelerator Systems

Several technical challenges posed by the collider's superconducting magnet support systems have been addressed in the past two years. Results of experiments, calculations, and studies increase our confidence that commercially available components can meet many of the requirements of these systems.

Vacuum

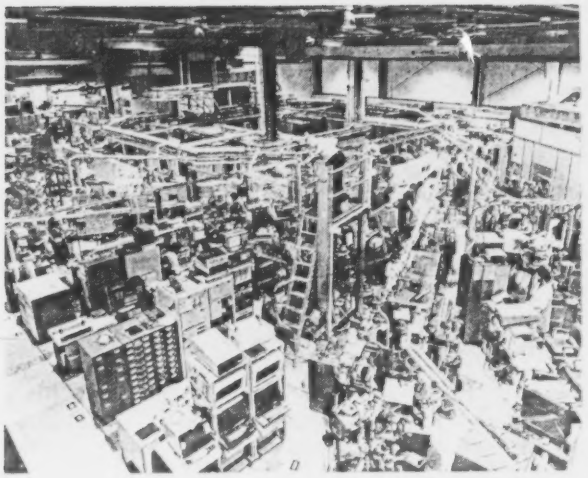
An exquisite vacuum must be maintained in the SSC beam pipe to reduce collisions with gas molecules that would deflect the protons from their proper orbit. To achieve the vacuum, the warm beam pipe is first pumped down with turbomolecular vacuum pumps and sealed. Then the magnet refrigeration system is turned on, and an ultra-high vacuum—a millionth of a billionth of normal atmospheric pressure—is achieved by the pumping action of the liquid-helium–temperature beam pipe walls. However, when the beam is present, synchrotron light emitted by the 20-TeV protons will desorb gas molecules from the beam pipe, increasing the gas density in the beam pipe.

It was necessary to know whether the density of the desorbed molecules would spoil the vacuum sufficiently to shorten the lifetime of the SSC proton beam. Because little was known about photon-induced gas desorption from surfaces at cryogenic temperatures, a series of experiments was undertaken.

The first experiment, performed at the National Synchrotron Light Source at Brookhaven National Laboratory, determined that desorbed gases could build up and slightly reduce beam lifetime after long periods of high-intensity beam. The experiment also indicated the cure for this problem: occasional slight warmings of the beam pipe followed by conventional turbomolecular pumping remove the accumulated gases.

This work was verified by using a different technique to measure photon-induced desorption in an experiment mounted at the Synchrotron Radiation Center in Stoughton, Wisconsin.

Photon-induced desorption with and without a weak magnetic field was investigated at the Japanese National Laboratory for High-Energy Physics. Desorption in a magnetic field was a tenth of desorption without no magnetic field. Since almost all desorption within the SSC beam tube occurs in the magnetic field of the bending magnets, this result indicates that desorbed gases will not be a problem for the SSC.



Vacuum ultraviolet ring at the National Synchrotron Light Source at Brookhaven National Laboratory, Long Island, where two experiments were mounted to learn more about the desorption effects of synchrotron radiation in the SSC. (Courtesy of Brookhaven National Laboratory)

Cryogenics

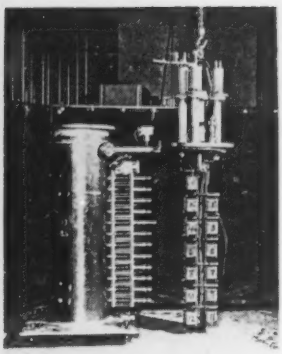
The SSC will have the world's largest helium refrigeration system, nearly 10,000 superconducting magnets that must be maintained at the chosen operating temperature of 4.35 K, under a variety of operating modes and over distances.

To allow construction, installation, and commissioning to be performed in parallel, we have designed a system of units capable of independent operation but interconnected for redundancy. The present design calls for ten cryogenic units or sectors for the collider ring, plus an overall 5 percent for the injector. Each sector requires a cryogenic refrigeration capacity of the equivalent of about 2500 watts at 4.35 K, within current industrial practice.

However, the SSC cryogenic system presents a significant challenge in process control. It must not only produce and deliver the cooling required for normal operation of the collider, but also accommodate both transient thermodynamic conditions as cooldown, magnet quench, and beam acceleration. In addition, it must function reliably to meet the design requirement that beam be available for experiments at least 85 percent of the time.

With help from industry, the Central Design Group has underway two computer simulations of the SSC's cryogenic system. In the first, refrigeration plant concepts are investigated by static process simulation; a base operating plant flow sheet was developed and studied in a variety of operating modes. More recently, we began to model the behavior of the cryogenic system as a whole: the refrigeration plant design is completed and the behavior of transient conditions in one cryogenic sector of the collider and the cryogenic interactions between sectors can be analyzed.

Both simulations are important for a final engineering design. Modeling confirms the adequacy of plant design concepts, aids instrumentation and control systems design, and assists the task of writing specifications. It also provides a tool for judging design proposals and helps assure that a self-consistent set of specifications has been produced.



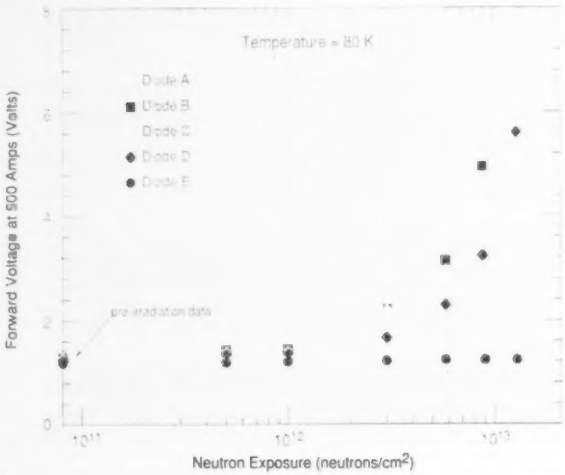
Diode evaluation at the Texas A&M University Nuclear Science Center Research Reactor. Commercially available cold diodes were placed in a specially designed cryostat that maintained all the diodes at the same cryogenic temperature while permitting electrical tests on individual diodes (above). The cryostat was placed in the reactor radiation cell, and reactor operation was suspended several times to allow measurements of diode forward and reverse characteristics as a function of neutron exposure. (Courtesy of Texas Accelerator Center)

Quench Protection Diodes

A reliable quench protection system is essential for superconducting accelerator magnets. If a superconductor is warmed sufficiently, current no longer flows through it without resistance. The phenomenon of going from the superconducting state to the normal resistive state is called quenching. A magnet in the resistive state would quickly overheat if it were not protected by immediately switching the current flowing through that magnet's coils to a bypass circuit.

The Conceptual Design specified a reliable active system of quench protection, in which room temperature diodes shunt a half-cell of six magnets. We are now pursuing a passive system in which power diodes inside the cryostat are connected in parallel with each magnet. This scheme is inherently simpler, and it minimizes the loads on the cryogenic system.

At the SSC, the diodes' location inside the magnet cryostat exposes them to the extremely low temperature of liquid helium, and to a relatively high flux of energetic neutrons. To test the proposed new quench protection system under these conditions, the Central Design Group, in cooperation with the Texas Accelerator Center, has studied the effects of low-temperature irradiation on several commercial semiconductor power diodes. Preliminary results of these experiments suggest that a passive quench protection system, using power diodes at liquid-helium temperature to bypass the current if a magnet quench occurs, is feasible for the SSC. We have identified a commercially produced diode, for which no systematic failures due to radiation damage are expected in the SSC lifetime.



Forward voltage at 500 amperes as a function of neutron exposure for 5 commercially available diodes tested at the Texas A&M Nuclear Reactor. Diode E is significantly more radiation resistant, showing only a 6 per cent increase in its forward voltage after being exposed to about 1.2 x 10^{13} high-energy neutrons per square centimeter, the expected dose from about 100 years of SSC operation.

Tunnel Radiation Environment

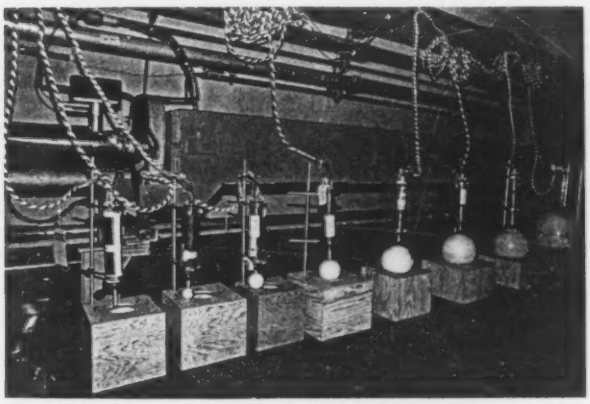
Although radiation in the SSC arcs is insignificant from an environmental standpoint, it was important to ensure that it would not damage the extremely sensitive solid-state electronic devices that reside in the tunnel.

To understand the tunnel radiation environment, calculations were done and their basis verified experimentally.

Specialists from the Oak Ridge National Laboratory calculated radiation resulting from particle losses in the Tevatron and SSC tunnels. The results indicated that ionizing radiation is relatively unimportant, that the neutron spectrum peaks near 1 MeV, and that the calculations of neutron fluence in the SSC tunnel outside the magnets are self-consistent.

A Central Design Group collaboration with Fermilab and Lawrence Berkeley Laboratory then performed experiments in the Tevatron tunnel to check the calculations. Nitrogen gas was leaked into the center of a 12-meter-long warm straight section of the Tevatron beam pipe. A neutron spectrometer and other detectors were deployed to measure rates as a function of pressure in the beam pipes.

The agreement of experimental data with the simulation calculations for the Tevatron allows us to extrapolate the simulation calculations to SSC energies with confidence. From this work, it was possible to determine how to shield sensitive equipment, such as correction coil power supplies and microprocessors.



The Lawrence Berkeley Laboratory Bonner sphere neutron spectrometer deployed in the Tevatron tunnel at Fermilab. Polyethylene spheres of different radii surround small lithium-iodide detectors. Because neutrons lose energy as they travel through the plastic, each detector registers only neutrons above a known energy, and spectral information can be obtained. (Courtesy of Alex Elwyn, Fermi National Accelerator Laboratory)

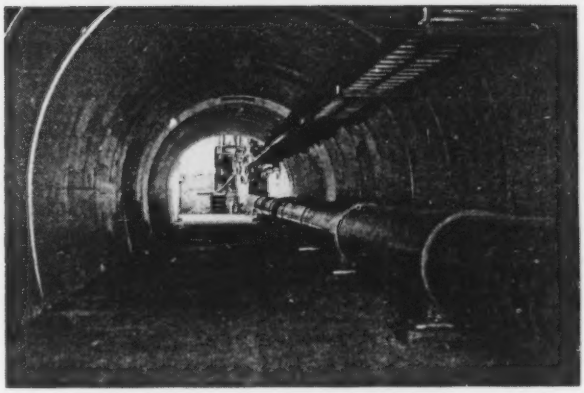
Cryogenic Safety

Safe operation is an important goal of the SSC design. Operational safety comes from the combination of design measures, hazard monitoring, operational procedures, and worker training.

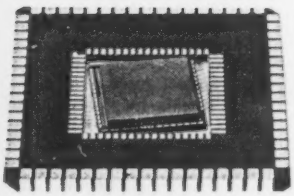
The use of cryogenic fluids in the restricted underground spaces of the SSC not only increases the potential oxygen deficiency hazard, but also introduces hazards associated with contact if spilling occurs. Design measures and operating procedures for mitigating these hazards have been developed for the Tevatron at Fermilab and at Brookhaven National Laboratory. The Central Design Group has commissioned three studies of cryogen spills, to suggest safety measures that can be incorporated into the design of the tunnel, the cryostat system, and the control system of the collider. As the engineering design progresses, further studies will be made to determine optimal designs and procedures.

System Test Plan

Some properties of the magnet system must be tested using strings of magnets. For example, the peak and average voltage, temperature, and pressure during a quench cannot be accurately determined by computer simulation nor by tests of single magnets. String tests will simulate a sector of the collider ring, allowing us to refine installation procedures and alignment techniques, study the operation of multiple magnets, and further develop and test the quench protection, cryogenic, and energy extraction systems. The magnets shown in the photograph on the right are ready to be tested.



The first two long magnets in the cell test at the Fermilab Experimental Research Facility. (Courtesy of Fermi National Accelerator Laboratory)



Measuring the particles produced in proton-proton collisions requires sensitive energy detectors and tracking devices. Shown above is a prototype pixel detector for particle detection. The detector (the top structure in the photograph) is bonded to an electronics read-out chip. In this example the detector consists of an array of 256 × 256 individual sensitive elements called pixels. Each pixel is individually bonded to amplifying and read-out circuitry. Pixels of approximately 0.001 by 0.001 inches are needed for the SSC. (Courtesy of Hughes Aircraft Company, Space Sciences Laboratory at the University of California, and the Stanford Linear Accelerator Center.)

The Detectors

To answer questions about rare processes, the SSC must have a high collision rate, so that enough unusual events will be produced to be confirmed and studied. About 100 million proton-proton collisions will occur every second at the SSC, some of which will produce hundreds of particles. Identifying these evanescent products requires measuring their energies and directions.

To achieve this goal, many-layered detectors surround the interaction region. Typically, closest to the collision point a detector with good spatial resolution tracks the many closely spaced particles emerging from the collision. Another layer measures the curved paths that electrically charged particles follow in a magnetic field. The energy and direction of both charged and neutral particles, or groups of closely spaced particles, are measured by the outer layers. In every layer, high spatial resolution is important to enable different products to be distinguished.

Storing and studying the data from every collision would, of course, be impossible. Sophisticated electronics and computer systems filter the data and discard phenomena that are already well understood. Only a handful of interesting collisions per second are recorded permanently.

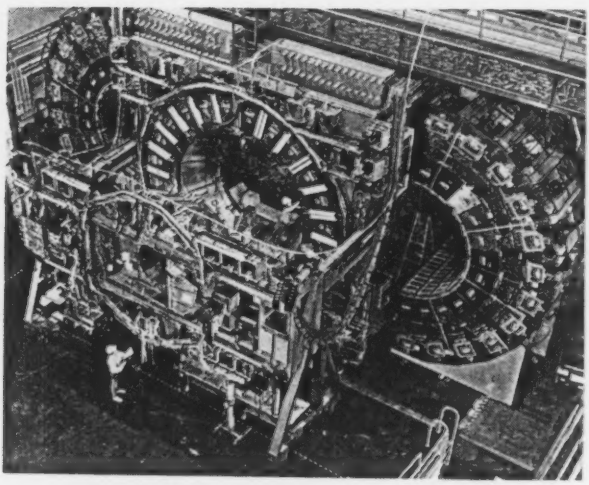
The SSC's high event rate, the large number of tracks per event (more than 100 particles can be created in a single proton-proton collision), and the need to resolve the tracks of particles that have similar energies and directions present a challenge to detector technology. To

meet this challenge, new detection techniques must be developed or older methods improved.

Detectors for the SSC must be colossal to trap the very high energy particles that the SSC will create: an SSC detector might weigh as much as 50,000 tons and be as tall as a 5-story building. Despite this enormous physical scale, research and development of detector technology can be carried out by small groups of physicists and engineers at universities and laboratories. Productive collaboration with industries involved in silicon detectors, advanced electronics and computing, scintillating materials, and many other areas is vital.

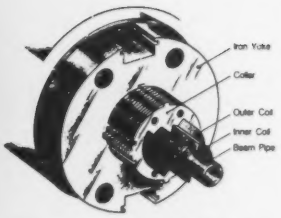
At present, detector research includes the development of high-speed, two-dimensional silicon detectors (pixel devices) and the associated "smart" readout electronics that record only selected data; custom integrated circuits for low-noise, high-speed applications; new, radiation-resistant scintillating materials; large-volume, high-field superconducting magnets for experiments; and new large-scale methods for precise measurement of particle energies and directions by calorimetry. Initial work is being pursued in more than a dozen universities and in five high-energy physics laboratories across the country, involving collaborations with scientists in Canada, Japan, and Europe. As the opportunity for university, laboratory, and industrial participation is large, the next two years will see a substantial increase in such efforts.

This reconstructed event from the Fermilab Tevatron shows how the tracks of particles produced in a collision reveal the particles' properties. The tracks of electrically charged particles curve in the detector's magnetic field: the tighter the curve, the lower the particle's energy. (Electrically neutral particles make no tracks. Their presence is inferred from energy deposition data.) A proton and an antiproton collided in the center of the starburst. Near the 12 o'clock position, colored bars indicate the energy deposited by a jet of charged and neutral particles in electromagnetic (red) and hadronic (blue) calorimeters. The track at 5 o'clock that appears straight is actually a very high energy muon; it penetrated thick layers of iron and registered in muon counters outside the main tracking devices. This collision produced a short-lived W boson plus a jet of other particles; the W disintegrated near the interaction point into the muon and a neutrino. (Courtesy of James Siegrist, Lawrence Berkeley Laboratory)



The collider detector at Fermilab. Its design and construction has been a joint effort by physicists and engineers from Japan, the United States, and Italy. (Courtesy of Fermi National Accelerator Laboratory)

The Magnets

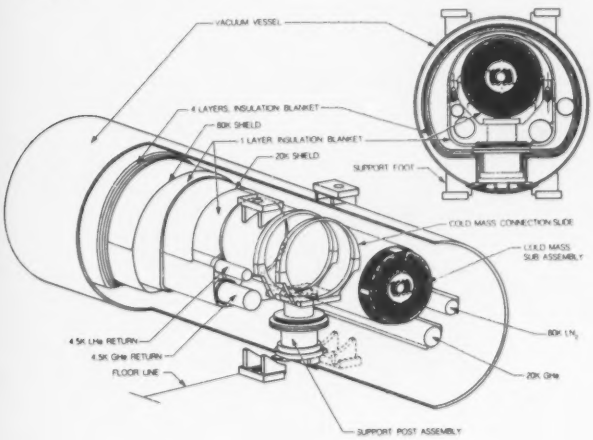


Cutaway perspective of the high-field superconducting dipole chosen for the SSC. Two superconducting coils surrounding the beam pipe are held in position by metal collars and surrounded by an iron yoke.

In 1983, the largest installation of superconducting magnets in the world began operating at a particle accelerator, the Fermilab Tevatron. That moment was historic, not only for physics, but also for technology: it marked the advance of superconductivity from a laboratory curiosity to a practical tool on an industrial scale. The Tevatron demonstrated that large-scale, factory-style manufacturing could be successfully employed to produce superconducting magnets. The federal government funded most of this development as part of our nation's basic research effort.

Next year, in preparation for commercial production of SSC magnets, we look forward to disseminating to industry the superconducting magnet technology that has grown out of SSC research and development.

Cross section of the SSC magnet and cryostat assembly, incorporating the latest design features. Multiple layers of superinsulation and an innovative support system minimize heat leakage into the cryostat.



The need to bend and focus the proton beam makes the magnet system a key element of the SSC. Two types of magnets are required: Dipole magnets, with one north and one south pole, guide the protons in their the proper orbit. Quadrupole magnets, with two north and two south poles, focus the protons into a needle-thin beam.

Magnet Requirements

The operating field strength, the aperture of the beam chamber, and the length of the magnets were chosen on the basis of optimization studies that included both technological and economic factors. In addition to producing the desired magnetic field strength throughout the chosen aperture and length, the magnets must

- endure many transitions (quenches) from the superconducting to the normal resistive state, while remaining relatively cool;
- achieve maximum operating field with few training cycles (where the magnet quenches before reaching full field);
- withstand the radiation of the tunnel environment;
- have uniform, precisely aligned magnetic fields;
- be extremely well insulated electrically;
- be capable of meeting all other requirements through more than ten thousand cycles to full field;
- be able to sustain more than twenty thermal cycles from room temperature to 4 degrees above absolute zero;
- minimize heat transfer from outside the cryostat to the cryogenic region enclosing the magnet core.

The ultimate engineering design, to be made in industry, must also be readily manufactured.

To choose a conceptual design for the collider dipole magnets, five different magnet styles were investigated. At several laboratories around the country models were built and tested. It was found that magnets with a field strength between 6 and 7 teslas would be most cost-effective for the SSC. Weaker fields, which curve the proton orbit less, mean a longer, more expensive tunnel; stronger fields increase the forces that act on the magnet coils and structure, and the savings from having a shorter tunnel are swallowed by the more expensive supports required.

The goal of producing the most important physics results for the fewest dollars means that, in addition to the high field strength, the magnet design has to be aggressive in two other respects: length and aperture size. The longer the magnets, the fewer expensive interconnections are required. Long magnets, however, present more mechanical challenges. A smaller beam pipe aperture in the center of the magnets diminishes the area of the magnet cross section, and thus reduces the amount of materials required to build the magnet, but a smaller aperture places more stringent demands on tolerances.

The final conceptual design for the collider dipole magnets specifies a field of 6.6 teslas, produced by a current of 6500 amperes in a niobium-titanium superconducting coil at a temperature of 4.3 degrees above absolute zero. The 17-meter-long magnets snugly cradle the 3.3-centimeter-internal-diameter beam pipe.

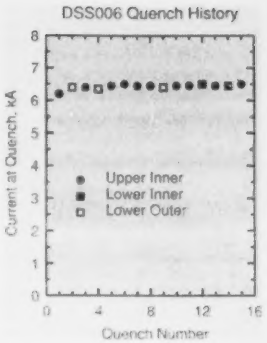
Development and Testing

Initially, a series of six model magnets of the chosen style, 4.5 meters long (about 15 feet), were made and tested. These met the necessary design criteria and provided important information for the full-length models (17 meters, or 55 feet long) now being developed.

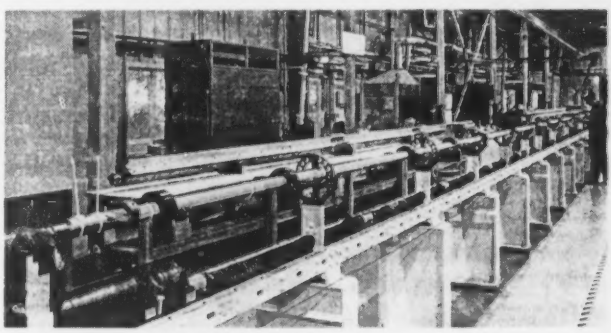
Four long model magnets have been built and tested. Many of the required criteria have been met in these models. Further improvements are needed in the number of training cycles and in the electrical insulation. To secure these improvements, a systematic program of analysis, tests, and development is under way.

To help analyze possible improvements and to verify the quality of components supplied by various vendors, a series of short magnets built at Brookhaven National Laboratory and Lawrence Berkeley Laboratory is also being tested.

Concurrently, the Central Design Group and the national laboratories hope to begin a magnet technology transfer program involving industry, which ultimately will lead to industrial engineering and manufacturing designs. The planned schedule allows industrial production tooling to be developed, magnet prototypes to be built, and the needed production rate to be demonstrated in time for industry to begin mass production of the magnets in 1992.

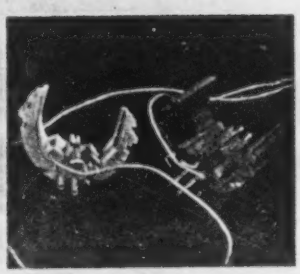


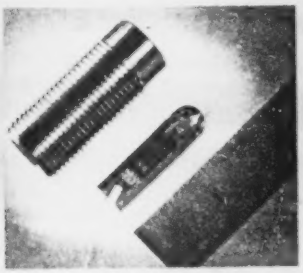
Magnet currents at which quenches were induced during testing of a recent 1.8-meter model dipole. The absence of training quenches and the stability of the quench current are desirable characteristics.

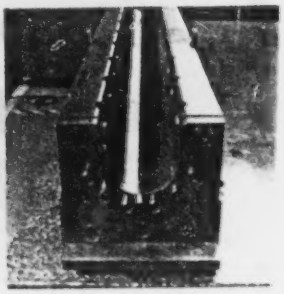


One of the first full-length model SSC magnets being built at Brookhaven National Laboratory. Here, the coils for the 55-foot-long magnets are being wound on a supporting mandrel to ensure that the conductor is accurately placed. The coils are then heat-molded to a precise size and clamped around the beam pipe by high-strength stainless steel or aluminum collars. The coil package is mounted within a laminated iron yoke. The resulting magnetic "cold mass," which weighs eight tons, is supported and cooled to liquid helium temperatures in a cryostat. (Courtesy of Brookhaven National Laboratory)

Magnet Development and Testing

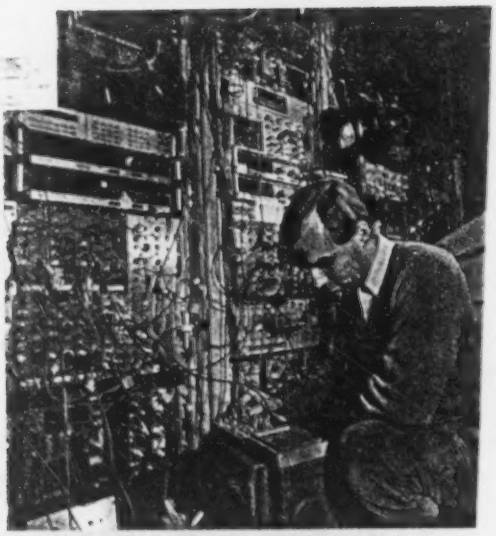






A strain gauge assembly built into the magnet collars for continuous measurement of internal coil pressure during magnet excitation. (Courtesy of Brookhaven National Laboratory)

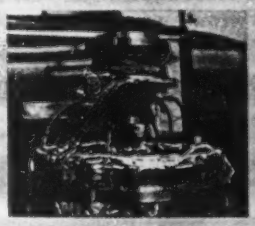
Magnets for a measuring probe developed at Brookhaven National Laboratory to measure magnetic field quality. The device consists of magnetic field probe(s) and probe positioning mechanism(s) housed in a 3-in.-diameter, 20-ft-long, Mother-Bar mounted on the resistor assembly of the 4-m-diameter magnetic field. (Courtesy of Brookhaven National Laboratory)



Magnet Test Facility control room at Fermilab. Thermal, mechanical, and electrical instrumentation installed in each of the model magnets is connected to a computer data acquisition system. The data can be accessed by scientists and engineers at the participating laboratories as the magnets are being tested. (Courtesy of Fermi National Accelerator Laboratory)

Detail of test instrumentation on a long model magnet. In support of the ultimate engineering design, detailed analyses and measurements of magnets' internal mechanical and electromagnetic behavior are performed. (Courtesy of Fermi National Accelerator Laboratory)

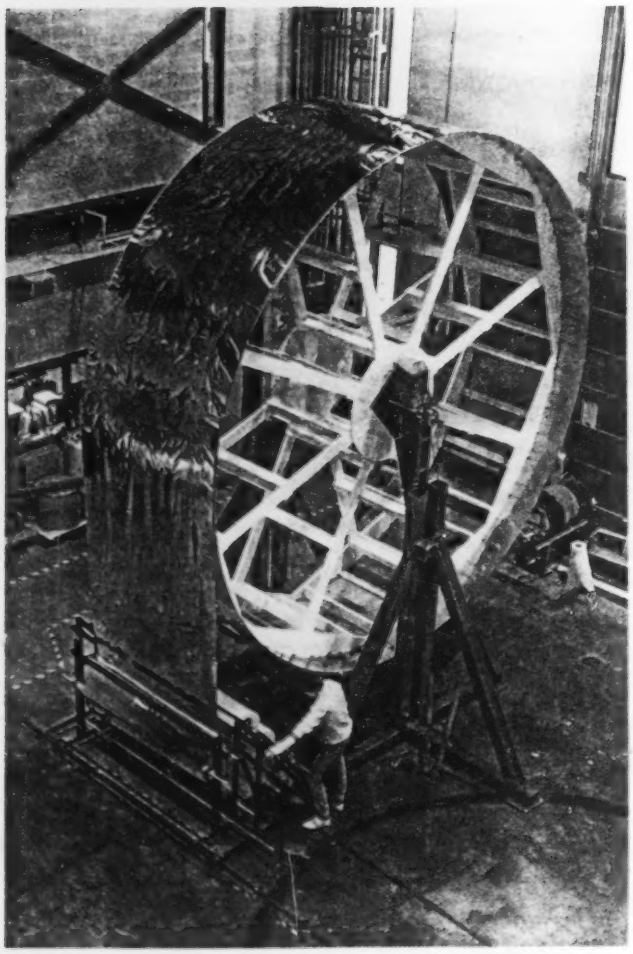
Cryostat



A cold-mass support post being prepared to have its thermal performance measured. After assembly, the post is placed in a special cryostat in which the temperature of the ends and at the 80 K thermal interchanges can be controlled and measured. The heat flow and temperature distribution across the post can be measured extremely accurately by varying a heater at the warm end of the post and measuring the change in temperature across a calibrated sample at the cold end. This device is also used to test superinsulation and many subassemblies of SSC cryostats. (Courtesy of Fermi National Accelerator Laboratory)

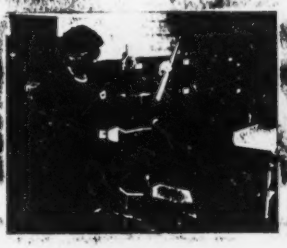
The magnet core must be maintained at superconducting temperature, isolated from the ambient environment by a cryostat providing mechanical support and thermal insulation. Although the initial cryostat performed satisfactorily during the early magnet test program, a recent Fermilab design simplifies the cryostat's manufacture, installation, and alignment.

Successful initial tests of the improved design have demonstrated that it meets SSC thermal and mechanical requirements. For example, the heat leak into the 4.3 degree portion of the magnet was measured at less than 0.2 watt over the 17 meters, even less than the design requirement of 0.3 watt. This important magnet element will soon be available for industrial engineering, construction of prototypes, and manufacturing design.

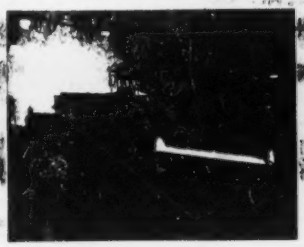


This wheel winds the aluminized Mylar™ superinsulation blanket for the SSC model cryostats. (Courtesy of Fermi National Accelerator Laboratory)

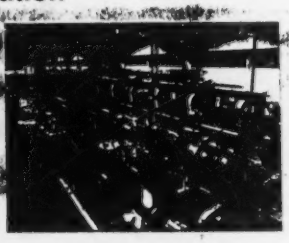
Superconducting Cable Fabrication



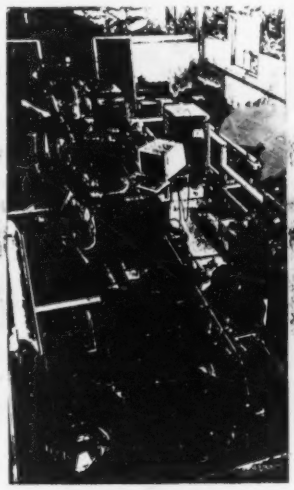
1. One method of fabrication begins with a rod of niobium-titanium an eighth of an inch in diameter. The rod is sheathed in a hexagonal copper tube; 7000 rods are placed in a copper extrusion can. (Courtesy of Intermagnetics General Corporation)



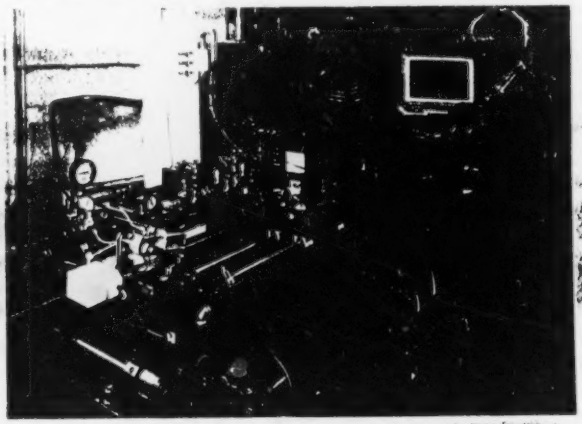
2. In one process, the sealed and evacuated can, or billet, is 12 inches in diameter and 30 inches long. Extruding the billet into a cylinder 3 inches in diameter and 40 feet long in preparation for rod drawing also bonds the materials into a true composite. (Courtesy of Climax Performance Materials Corporation)



3. The extruded billet is drawn in multiple passes to wire size. During the final drawing, it is heat treated to optimize the wire's current-carrying capacity. The wire is then twisted to minimize induction effects. Each billet yields 250,000 feet of multifilamentary wire 0.03 inch in diameter. (Courtesy of Intermagnetics General Corporation)

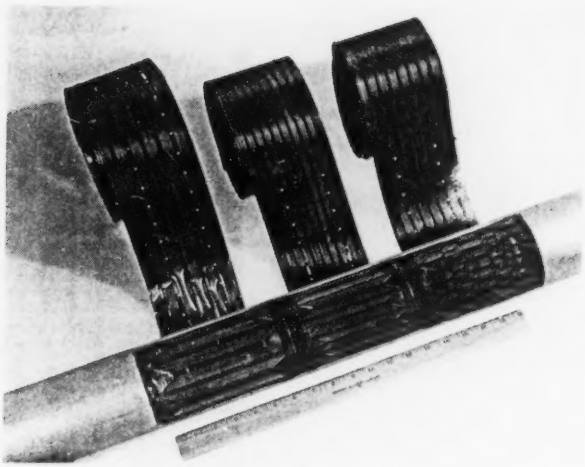


4. The wire is wound into a nearly flat, compacted cable of either 23 or 30 strands. The cable is made with a slight keystone angle to permit the conductor to be readily stacked into the arch shape of the magnet coil. (Courtesy of Lawrence Berkeley Laboratory)

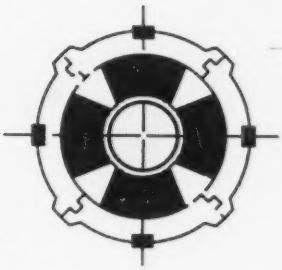


5. Laboratories assisting with the development of SSC cable have generated new designs and improved procedures for winding wire into cables. To ensure that the cable is being produced to the exacting tolerances SSC magnets require, this in-line cable measuring machine was developed by Fermilab. It measures cable thickness under 5,000 pounds per square inch of pressure to within 0.0001 inch at a rate of 30 feet per minute. It simultaneously measures cable width and keystone angle. The new machines are now installed on SSC R&D cable production lines. (Courtesy of Lawrence Berkeley Laboratory)

6. Finally the cable is insulated with a spiral wrap of Kapton™ film and a layer of glass fiber tape impregnated with epoxy, to ensure the structural rigidity of the coil. An enlarged cross section of 23-strand superconducting wire for SSC magnets is shown at the left. The cable actually measures 0.366 inches left to right. (Courtesy of Lawrence Berkeley Laboratory)

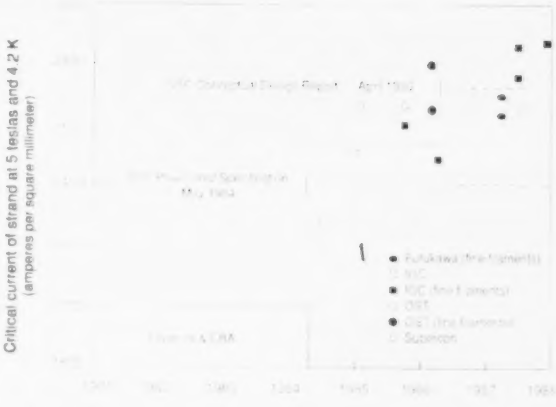


Laboratories involved in SSC R&D are working closely with an industrial vendor to develop field correction coils mounted on the beam tube. They can be powered in response to signals from the proton beam itself during operation. From left to right, the trim coil sheaths produce sextupole, decapole, and octupole magnetic fields. (Courtesy of Brookhaven National Laboratory)



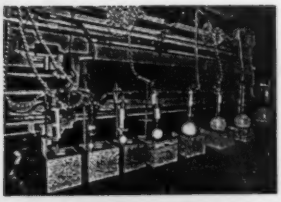
A cross section of the collared coil assembly of a quadrupole or focusing magnet. Note the four coil segments which power alternate north and south poles. The SSC quadrupoles are now at an early modeling and concept design stage; 1400 will eventually be industrially produced.

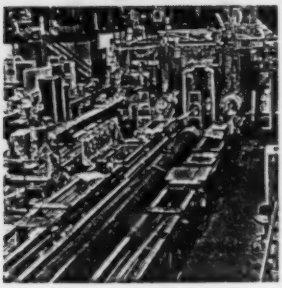
Industry is already heavily involved in the heart of the magnets, their superconducting cable. American vendors, experts at the University of Wisconsin, at Lawrence Berkeley Laboratory, and at the other participating laboratories have made possible a steady improvement in the performance of industrially produced niobium–titanium composite wires. Advances include a more homogeneous basic alloy, more effective mechanical working and heat treatment cycles, and improvements in the tools and methods used to form the wires into cables. Cables that fully meet SSC requirements are now being produced; full-scale, industrial mass-production will soon be possible.



Improvement in current-carrying capacity of commercially produced wire as a result of research and development for the SSC. Wire for the Tevatron and the CBA project carried 1800 to 2000 amperes per square millimeter under test conditions. After several manufacturing cycles, industrial suppliers have produced wire surpassing 2750 amperes per square millimeter. Superconducting wire is of worldwide interest, as similar improvements abroad show. (Courtesy of Lawrence Berkeley Laboratory)

The Next Steps





Over the past four years, an extraordinary scientific instrument, the Superconducting Super Collider, has progressed from concept toward reality. The talents and dedication of scientists, engineers, and technicians in national laboratories, universities, and industry have made possible the accomplishments highlighted in the preceding pages. During the coming year, the careful planning and intensive research-and-development phases of the SSC project will be ready to expand rapidly in support of the first construction activities.

Ahead lie the completion of systems engineering, the optimization of accelerator operations, and the engineering design of experimental halls and detectors. Major work will begin on engineering designs of the accelerators, and prototypes of components will be built and tested.

The future will see the transfer of SSC dipole magnet technology to industry, opening the way for superconducting accelerator magnets to be industrially produced. The enhancement of industrial fabrication techniques for superconducting cable will continue. Strings of full-length model magnets will exercise the collider's cryogenic, electrical, and vacuum systems. Prototype dipoles will be tested under conditions that simulate twenty-five years of SSC operation, to uncover and eliminate design flaws and potential failure modes.

The worldwide physics community will participate in the creation of the Supercollider Laboratory by giving detailed definition to the experimental program and by further refining the art of detector design and construction.

The lure of the unknown and the scientific puzzles that define the frontier of our understanding draw us onward in our exploration of the ultimate secrets of matter and energy. The voyages of discovery into the uncharted reaches of inner space on which the Supercollider will carry us will be the eventual rewards of the SSC R&D program.

The Superconducting Super Collider R&D program is supported by the U. S. Department of Energy. Universities Research Association, a consortium of 66 research universities, is responsible for carrying out this program, which is coordinated nationally by the SSC Central Design Group, located at Lawrence Berkeley Laboratory. Additional copies of this booklet and further information about the Superconducting Super Collider may be obtained from the SSC Central Design Group, c/o Lawrence Berkeley Laboratory, MS 90-4040, 1 Cyclotron Road, Berkeley, California 94720, (415) 486-4772.

American Chemical Society

OFFICE OF THE
PRESIDENT

Gordon L. Nelson
President-Elect, 1987
President, 1988
Immediate Past President, 1989

1155 SIXTEENTH STREET, N.W.
WASHINGTON, D.C. 20036
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April 12, 1988

The Honorable Wendell H. Ford
Chairman
Subcommittee on Energy Research and Development
Committee on Energy and Natural Resources
United States Senate
Washington, D.C. 20510

Dear Mr. Chairman:

The American Chemical Society appreciates this opportunity to present its views on The Department of Energy's FY 1989 budget request and to offer some specific comments on support for research in the Office of Basic Energy Sciences (BES).

The United States is the recognized world leader in many energy and chemical technologies; but this leadership is being challenged. Long-term basic research activities supported by the BES program are essential to continue the Nation's leadership in these areas. The knowledge base for new technologies and future energy options springs from BES-supported research, thus helping to ensure that the U.S. maintains its energy and economic security and meets the challenge of increased international competition in science and technology.

Not only does the operating budget of BES fund long-term basic research, it also supports high-quality national research facilities heavily used by researchers in industry, government, and academe. These user facilities provide valuable opportunities for technology transfer and for the training of scientists and engineers. In fact, over 3,400 researchers from industry, government, and academe used these DOE research facilities last year.

The Society notes the proposed increases in FY 1989 for the operating expenses of the Chemical Sciences, Materials Sciences, Energy Biosciences, and Engineering and Geosciences programs within BES. However, a look at the historical budgets indicates that growth in these areas has been too low. This fact was highlighted in the conclusions of the Basic Energy Sciences Advisory Committee's December 1987 report, which stated that "....the funding of BES has not expanded enough to deal adequately with the needs, and certainly not the opportunities, facing the Nation and the DOE."

Budget Recommendations

One of the primary missions of the Department of Energy is to assure future access to abundant and clean sources of energy. Over 90 percent of the Nation's energy is based on chemistry. Given (1) the importance of chemically-related basic

research to the DOE mission, (2) the need to have a secure energy-based economy, and (3) the trends in funding for the Basic Energy Sciences program, the American Chemical Society recommends:

- A 20 percent increase over FY 1988 be allocated to the operating budget of the Chemical Sciences Division, from $88.2 to $105.8 million.
- A 15 percent increase over FY 1988 be allocated to the operating budget of the Materials Sciences Division, from $126.0 to $144.9 million.
- A 15 percent increase over FY 1988 be allocated to the operating budget of the Energy Biosciences Division, from $20.3 to $23.3 million.
- A 20 percent increase over FY 1988 be allocated to the operating budget of the Engineering and Geosciences Division, from $31.4 to $37.7 million.

The Society recognizes the urgency of budget constraints. However, in the context of the promising research directions available, the budget growth being proposed is modest when compared to the potential benefits to the Nation's economic future.

Research Directions

Exciting areas of research now exist with definite promise for payoffs to the U.S. economy and the energy picture, and should be pursued. Opportunities in the chemical sciences and engineering have been identified as key to America's energy future, in briefings and reports of the National Research Council; the Committee on Science, Engineering, and Public Policy (COSEPUP); and various government advisory groups.

The soon-to-be published National Research Council Report, "Frontiers in Chemical Engineering: Research Needs and Opportunities," identifies promising research thrusts in the areas of In-Situ Processing of Energy and Mineral Resources, Liquid Fuels for the Future, Advanced Computational Methods and Process Control, Surface and Interfacial Engineering, Microstructured Materials, and Biotechnology and Biomedicine. These areas have research and budget implications for the DOE Office of Basic Energy Sciences, particularly its Divisions of Chemical Sciences, Materials Sciences, and Engineering and Geosciences, as well as elsewhere in the Department.

Similarly, the 1985 National Research Council Report, Opportunities in Chemistry, showed that reaction dynamics, catalysis, combustion, corrosion, and plant sciences are essential to DOE's energy mission. The Department still has not responded adequately to the recommendations in this Report with new thrusts and new monies. These research areas relate directly to the chemical research supported in the Divisions of Chemical Sciences, Materials Sciences, and Energy Biosciences.

The Department of Commerce, in a recent review of emerging technologies and their impact on the economy (The Status of Emerging Technologies: An Economic/Technological Assessment to the Year 2000, June 1987), has concluded that, in order

to remain competitive internationally, American industry by the turn of the century must master such key materials areas as superconductors, polymer composites, optical electronics, and advanced microelectronics. Materials science is at the heart of nearly all technologies that pace economic productivity, from industrial technologies to biomedical science to electronics to transportation to energy conversion. Chemists and chemical engineers must be given adequate budget support to play a key role if the U.S. is to meet this national agenda.

High-temperature superconducting materials--substances that lose all resistance to electricity below a certain critical temperature--could yield technological leaps in telecommunication, electronics, power generation and transmission, aerospace, and medical technology. The recent Report of the Research Briefing Panel on High-Temperature Superconductivity, COSEPUP, 1987, characterizes international competition in this field as intense, but indicates that the U.S. competitive position is good. The report concludes, however, that "Japan offers perhaps the strongest long-range competitive threat to the U.S. position." In view of this intense global competition, ACS believes that it is critical that research in high-temperature superconductivity be placed in an environment which will ensure long-term sustained funding. Here again, chemists and chemical engineers can play key roles in advancing an understanding of the properties of high-temperature superconducting materials, in the search for additional stable superconducting materials, and in devising chemical processes and methods for their wide application. An infusion of additional funding is urgently needed to continue the momentum of ongoing research. DOE's Basic Energy Sciences Advisory Committee has recommended that $30 million be added to the BES FY 1989 budget request for such research to be conducted in the Materials Sciences, Chemical Sciences, and Engineering and Geosciences programs.

Although the Society recognizes that there are high-temperature superconductivity initiatives in the budget request, it is not entirely clear from the budget documents that the research will be funded with new FY 1989 dollars. Therefore, it appears to us that the requested increases to BES programs are inadequate to pursue these and the other promising areas of research outlined above.

Benefits of Research

Research funded by the Chemical Sciences Division continues to contribute to fundamental knowledge about energy-related processes and developments--knowledge that has benefitted the Nation and has resulted in patents and commercialization.

An example of such success is the work of E. S. Yeung, Professor of Chemistry at Iowa State University and Senior Chemist in the Ames Laboratory, who has developed a new measurement technique. Dr. Yeung, 1987 recipient of the ACS's Award in Chemical Instrumentation, has combined the powerful laser with other scientific techniques to solve real life problems in energy generation, medicine, the pharmaceutical industry, and semiconductor fabrication. His patented micropolarimeter can record levels of optical activity much lower than those of previous measurements. He has applied this new methodology to determine optical activity in shale oil, coal, and coal extracts. The extent and nature of the optical activity, a measure of the right-handed or left-handed configuration of certain molecules, gives information about the age of the fossil materials and their geological origin.

Another application of this technique with far reaching consequences is its use in determining the level of blood cholesterol. High cholesterol has been correlated with atherosclerosis and heart disease. Current measurement techniques determine a single figure, while two forms of cholesterol, free and bound, are known to exist. The culprit in heart disease is thought to be the bound form. Yeung's new technique distinguishes between the two forms and measures each, as well as the total. The method is so sensitive that it improves on available commercial techniques by a factor of a thousand.

DOE-BES funded researchers also are attempting to make more of our energy resources economically usable. Natural gas, predominantly methane, is often found contaminated with nitrogen. It then has poor heating value and is less economically efficient to use. Chemical engineers at the University of Texas at Austin have prepared a new series of membranes which preferentially pass nitrogen rather than methane. This new process removes the undesired nitrogen, thus increasing the heating value of the remaining natural gas. The research looks promising and is being expanded to search for even more effective membrane materials.

Academic Research Support

The Society commends the Department of Energy for maintaining a strong component of its research at universities since they play a vital role in the research process. Currently, 34 percent of the BES research funds go to universities. The larger part of the total BES operating budget supports major facilities and research at national laboratories. Although the major facilities are heavily used by academic and industrial scientists and engineers, the balance among DOE research performers should be reviewed from time to time so as to determine the best mix to serve the Nation's interests.

The ACS believes that the University Instrumentation Program under the Office of Energy Research (OER) is underfunded. This subprogram was established to alleviate the pressing need among DOE researchers for state-of-the-art instrumentation costing over $100,000 each, and to allow graduate students to be trained with sophisticated research equipment. It also enables university researchers to employ "innovative" instrumentation on energy problems. This program was only able to support thirty awards in FY 1986. Strong proposal pressure coupled with flat funding over the past three years will allow this program to make only eighteen to twenty awards in FY 1989. In order to relieve this backlog of instrument needs in the academic community, the ACS recommends that Congress increase the University Instrumentation Program in FY 1989 by 50 percent to $7.5 million.

OER also reaches out through its University Research Support Program to provide research and educational opportunities at National Laboratories for university faculty, graduates and undergraduates, as well as high school science students and teachers. The Society supports the proposed increases for the semester-length research appointments at five science education centers located at DOE multiprogram laboratories. The ACS also supports the increases in the budget for educational opportunities for undergraduate science and engineering students, for elementary and secondary science students, and for high school teacher research programs. Our

national well-being rests on a strong science and engineering work force. The ACS is pleased to see that DOE is playing a role in enhancing the quality and increasing the number of young people interested in pursuing energy related scientific and technical professional careers.

The ACS hopes these recommendations will assist your Subcommittee in its deliberations on the DOE budget for FY 1989. The costs of the recommendations outlined are modest compared to the benefits to DOE's energy mission and to the economy as a whole.

Sincerely yours,

Gordon L. Nelson

cc: Members, Senate Subcommittee on
Energy Research and Development

FUSION POWER ASSOCIATES
2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879
(301) 258-0545

STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT
OF THE
SENATE COMMITTEE ON ENERGY AND NATURAL RESOURCES

The effort to develop a fusion energy source has two important and equally essential parts. The first is, in the near-term, to demonstrate fusion plasma conditions of density, temperature and confinement time close to what is required in a fusion reactor. This effort includes developing the necessary technology and scientific understanding for those near-term achievements. The second important and essential element of fusion development consists of those programs required to ensure that fusion will be useful for some practical purpose in the long-term. These programs include concepts and concept improvements whose environmental, safety and economic features will make fusion a winner in comparison with competing technologies. Such efforts include the necessary systems studies and analysis, materials and technology development, new concept experiments, and fundamental physics and engineering research both at national laboratories, industries and universities.

Maintaining the necessary balance between these essential near-term and long-term aspects of fusion development has become especially difficult for both the magnetic and inertial fusion programs because of the decline of their budgets in recent years and the flat funding projections of the next several years.

In magnetic fusion, the near-term program consists primarily of the TFTR and CIT tokamak programs at Princeton, other large tokamak experiments, notably the DIII-D at GA Technologies and the Alcator program at MIT, and supporting theory and technology efforts. I would also include in this category the ITER design effort, although its payoff might well be categorized as "mid-term." These programs are clearly essential to fusion progress and deserve a certain degree of priority in allocating funds.

It is unfortunate that the funding projected to be available for the CIT project does not permit it to be built expeditiously. As I warned in my testimony last year, "Unless substantial increases are provided in FY 1989, I would anticipate schedule slippages and project cost increases." The budget submitted does not provide the increases required for expeditious completion either for CIT or for several other projects, notably the CPRF at Los Alamos and the Field Reversed experiment at Spectra Technology.

Of more concern than the FY 1989 submission however, is the out-year projections. The OMB and DOE appear to have agreed on a 5-year projected flat budget profile that, if implemented, not only results in delays and cost increases for CIT but also spells disaster for many, if not most, of the programs required for the success of fusion in the long-term.

Systems studies, necessary for guiding the magnetic fusion program toward practical products, are projected to decline to a paltry $2 million per year. These studies include the investigation of concepts and fuel cycles that would impact the environmental, safety and economic characteristics of fusion devices. Also, a recent subpanel of the Magnetic Fusion Advisory Committee recently warned in its interim report that the program currently in place and projected would not result in sufficient data by the year 2005 to permit an assessment of the environmental, safety and economic aspects of fusion to be made nor to allow a fusion demonstration reactor to be built using low-activation materials.

It is not unreasonable to ask the long-term payoff portions of the fusion program to tighten their belts a little in FY 1989 to help launch a major new near-term project like CIT. It is unreasonable, however, for the executive and legislative branches to expect to build a $400 million new facility without providing new money for a substantial fraction of its cost. A new project of this magnitude requires some real budget growth. This problem is not too acute in FY 1989 but will become very acute in later years if the 5-year projections of DOE and OMB are adhered to.

A similar problem has presented itself in the case of the inertial fusion program. This program has had good experimental success in the last few years and is now ready to begin design leading to construction of a "Laboratory Microfusion Facility." Unless real growth in funding is forthcoming, design and near-term R&D for such a facility will restrict our ability to fund those aspects of the inertial fusion program which have long-term civilian payoff. These programs include fusion reactor conceptual design studies and the development of laser and particle beam technologies that are capable of the high efficiency and repetitive pulsing necessary for a commercial power system.

Of immediate concern to this Committee should be the failure of DOE to request funds for the $27 million ILSE heavy ion fusion accelerator at LBL. Heavy ion fusion accelerator development has been a part of DOE's Basic Energy Sciences programs. As a consequence of DOE deciding not to fund the ILSE accelerator, they are also seriously considering abandoning heavy ion fusion accelerator development altogether. I urge this Committee to authorize the ILSE project and to direct the DOE BES office to continue this important program.

Fusion research and development is important to our nation's future. The fusion community appreciates the support it has received from this Committee over the years.

RONALD GARREN, M.D.
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CARMEL, CALIFORNIA 93923

TELEPHONE 624-1895
OR 422-6632

1988 MAR 15 PM 12:02

March 10, 1988

The Honorable J. Bennett Johnston, Chair
Committee on Energy and Natural Resources
360 Dirksen Senate Office Building
Washington, DC 20510

Dear Senator Johnston:

I am writing to you in order to enter the following testimony concerning fusion energy in the budget hearings of your committee.

Firstly, I want to express my position that funding for energy directed fusion research should be continued and enhanced. Fusion is one of our best hopes for a clean inexhaustable energy supply for the future. Money should be appropriated, not only for basic research, but for research on materials which can withstand the high energies of fusion and which do not become themselves radioactive.

Secondly, it is imperative that the current programs in both magnetic and inertial confinement fusion be supported in order that the timetables for break even and high gain experiments can be met.

Thirdly, inertial confinement fusion, which is currently being funded by the military out of the D.O.E., should receive funding also from the civilian sector which has an interest in I.C.F.'s energy producing potential as opposed to the military's interest which is solely weapons related.

Lastly, I would propose that funding be established for educating the public who have a naturally skeptical attitude towards any technology associated with the term nuclear.

Thanks for your attention.

Sincerely,

R. Garren

Ronald Garren, M.D.

RG/cmm

The Honorable J. Bennett Johnston, Chair
Committee on Energy and Natural Resources
360 Dirksen Senate Office Bui[illegible] 1988 MAR 21 AM 10:21
Washington, DC 20510

March 11, 1988

Dear Senator Johnston,

I would like to submit the following text as written testimony for the Congressional Budget Hearings. I am sending a copy to your subcommittee on Energy and Water Development as well.

I write on behalf of a growing number of citizens who are becoming alarmed at the lack of serious financial attention given to research and development of future energy sources, and to fusion energy, in particular.

I think we all agree that our country faces many short and medium term problems that Congress should support with solid financial backing. However, it seems that some of the longer term considerations Congress should be addressing are often put aside because more immediate problems beckon with greater urgency.

Numerous studies have focused on the problems of population growth, future energy demand, depletion of fossil fuels, feasibility of various energy sources to provide base power and the environmental and safety aspects of candidate energy sources.

While estimates may differ as to the size of the world population by the middle of the next century, even conservative estimates predict a doubling to ten billion people. Common sense tells us that our economy would be severely crippled if we continued to deplete our fossil energy resources. The toll taken on the environment by the enormous number of pollutants pumped into the air by this explosive increase in the burning of these fuels is incalculable. We resist considering the financial costs which would arise from irreversible damage to croplands, forests, water resources, atmosphere and even climate. In addition, fertilizers, medicines and all the other by-products of oil would become too costly to produce.

Various schemes have been devised for production of solar energy on a massive scale. Both practical and environmental considerations would prohibit us from utilizing this source alone to provide our base energy Nuclear fusion would be a strong candidate for an intelligent main power source, complemented by solar and other renewable, benign sources.

Physicists have shown that fusion-generated energy is possible. It offers attractive advantages in the areas of safety and protection of the environment. A recent report written by the Senior Committee on Environmental, Safety, and Economic Aspects of Magnetic Fusion Energy (ESECOM), chaired by Dr. John Holdren of UC Berkeley, stated that "magnetic fusion energy systems have the potential to achieve costs-of-electricity comparable to those of present and future fission systems, coupled with significant safety and environmental advantages."

The Honorable J. Bennett Johnston
March 11, 1988
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Energy IS an urgent problem, because the fusion R&D program will need 20 to 30 years of consistent, strcng funding before they can build us a test reactor.

There is no question that a true energy crisis will occur. The question is whether we will have prepared ourselves to meet it. We need a strong and directed national energy program -- that means vision and support from our Presidents, but, more importantly, from Congress.

May I refer you to the Congressional Report prepared by the Office of Technology Assessment, Starpower, the U.S. and the International Quest for Fusion Energy, and to the ESECOM Report, published by the Lawrence Livermore Laboratory for a complete discussion of the status of fusion R&D and its promise.

Thank you very much for accepting my testimony. I hope that your vote of confidence will translate into solid financial support of fusion energy research and development.

March 11, 1988

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March 14, 1988

The Honorable J. Bennett Johnston, Chair
Committee on Energy & Natural Resources
360 Dirksen Senate Office Building
Washington, D.C. 20510

Dear Senator Johnston:

I am submitting my letter to your office to be entered as written testimony during the budget hearings pertaining to fusion.

In light of the current energy utilization of the United States and the projected growth in both population and production needs, present means of production cannot possibly be sufficient. The need to investigate alternative sources is not new nor has it been successful as yet. Fusion must not only be included in this quest but placed in the forefront. The new developments in fusion physics have established clearly its feasibility. We must now support the fusion program more aggressively by increasing materials research and additional technologies, so that the current knowledge can be translated into production in the future.

Funding, as you know, is needed for public education, advanced fuel production research, and new schemes for confinement fusion such as "the reverse

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field pinch" and "the tandem mirror system." Funding must support both the magnetic and inertial confinement technologies, as they undoubtedly will have dual or combined purposes in the future.

We must also acknowledge the need to promote and maintain the academic base within our university systems to perform this highly specialized discipline.

Inadequate funding can do nothing but move the United States energy position to the rear. Advanced funding will turn our vision for the future into a reality in the 21st century.

Thank you for reviewing my position. Hopefully you will see the wisdom of the need for fusion in our future.

Sincerely,

Stephen D. Barkalow, D.C.

SDB/nb

Statement of the

Institute of Electrical and Electronics Engineers, Inc.

on

Fusion Energy Research

to the

Subcommittee on Energy Research and Development
Senate Committee on Energy and Natural Resources

This statement is made on behalf of the Energy Committee of the Institute of Electrical and Electronics Engineers (IEEE), the world's largest engineering society with over two hundred thousand members living and working in the United States. Approximately 80% of the membership is employed in industry, 10% in academic institutions, and 10% in various government laboratories and agencies. The Energy Committee, with representatives from 32 different technical societies within the Institute, is charged to develop the Institute's energy policy which is then reviewed and promulgated to the public.

We of the Energy Committee of IEEE continue to believe that fusion possesses a very high potential as a long-term energy source which has the advantage of a practically inexhaustible fuel supply which is accessible to all nations.

We again commend this Subcommittee on its strong support for the fusion effort over the years. This subcommittee has been instrumental in supporting the "burning core experiment," which was recommended by the Department of Energy's Magnetic Fusion Advisory Committee over the past years: the Compact Ignition Tokamak (CIT) is included in the FY88 budget with $15.5M ($8 of which is PACE-Plant and Capital Equipment.) This initiative will provide the data for the assessment of the effects of strong plasma self-heating and of a significant population of fusion products, one of the few remaining physics questions to be answered in currently planned experiments as input to the design of a prototypical fusion reactor operation.

In the past year, the fusion program has made considerable progress. On the path to the break-even deuterium-tritium (DT) fusion experiments in 1990, the Tokamak Fusion Test Reactor has reached a world-record plasma ion temperature of 300 million degrees Centrigrade. In addition, 27 megawatts of neutral beam power can now be injected into the plasma. An additional 7 megawatts of heating power from the radio frequency heating system will significantly enhance the machine's performance. The DT break-even experiments are planned in FY90-91.

Panel XVII of the Magnetic Fusion Advisory Committee emphatically endorsed carrying out the proposed TFTR experimental program, recognizing the importance of the mastery of the physics of alpha particle heating and the technical benefits of preparing and operating a tritium-compatible and radiation-hardened tokamak for the fusion program.

The progression from the scientific break-even experiments to ignition is consistent with an overall strategy that addresses the physics issues in achievable and responsible steps. TFTR provides a physics basis for plasmas in

a reactor-relevant regime with significant fusion power; CIT provides an analysis of plasma behavior when a significant amount of fusion power is absorbed in the plasma. The next challange after CIT is the development of an engineering machine (ITER) that incorporates the equipment for processing the fusion output power and producing electricity. The CIT experimental results provide the plasma database for the operating phase of the proposed ITER project.

To develop fusion as a viable alternative for energy production in the first half of the twenty-first century, the program's momentum must be sustained. The fusion program has experienced declining budgets since FY85, as shown in the following table.

BUDGET HISTORY OF FUSION*

Fiscal Year	Fusion Budget-$M*	% Change rrom Prior FY
FY'82	451.2	
FY'83	461.3	2.2
FY'84	466.0	1.5
FY'85	437.0	-6.6
FY'86	365.4	-16.4
FY'87	345.4	-5.5
FY'88	335.0	-3.0
FY'89 (proposed)	360.0	7.5

*All dollar values are in "as year" dollars

As shown, budgets to support the new initiative (Compact Ignition Tokamak and ALCATOR C-MOD), the operation of the Tokamak Fusion Test Reactor, the Advanced Toroidal Facility at ORNL, the DIII-D at GA Technologies, and the Microwave Tokamak experiment at Lawrence Livermore National Laboratory as well as other smaller experiments, the Development and Technology program, and the design work for the International Thermonuclear Experimental Reactor (ITER) will require an increase in FY89 and beyond. The Department of Energy FY 1989 Congressional budget request shows such an increase.

The OFE (DOE Office of Fusion Energy) financial guidance for CIT (shown in the following table) prolong the CIT schedule, with experimental operation beginning in FY 96 and DT operation in FY98.

OFE FINANCIAL GUIDANCE FOR CIT

FY'89	$27M
FY'90	$46M
FY'91	$64M
FY'92	$95M
FY'93	$101M

This schedule imposes a five-year interruption in front-line experimental research from the end of TFTR operations to the start of CIT operations. This 5-year gap is so long that continuity in and the momentum of the U.S. research program will be lost, the assembled research team may lose critical members, and the United States may slip from its position as a leader in the international fusion program.

The addition of near-term funding would accelerate this schedule and shrink the interval. We recognize that there is extraordinary competition for funds this year, but we ask the Subcommittee to review the magnetic fusion program results, its potential for providing an internationally distributed source of electric power and compare this promise with the competition. While it is difficult to predict the actual future energy requirements and environmental guidelines, the IEEE believes that it is important that the world of the twenty-first century have a number of technologies available for commercialization. Creating such an opportunity for choice should be the long-term driving force for developing fusion at an appropriate pace.

Over the past three decades, the American people have supported fusion with funding totalling $4.5 billion. The goal of scientific break-even is at hand and the achievement of ignition is on the horizon. In order to best capitalize on this investment and afford the opportunity of choice to the next generations, the fusion program should be sustained.

We in the United States currently have a strong scientific community that is on a par with that of other nations pursuing the fusion energy option. International collaboration appears to be a realistic possibility if our domestic program is sufficiently strong to attract other nations who are invited to collaborate with us. Hence, in this international arena, the IEEE in its own position paper on fusion power, dated November 24, 1985, states "the IEEE urges the continuance of broad areas of international cooperation in promoting fusion power research." We endorse open international scientific collaboration and support of a domestic program of size comparable to that of other nations.

In summary, the Institute suggests two measures:

- That the subcommittee examine and accelerate the proposed funding for CIT to retain the national technical staff which has been assembled and to sustain the momentum of the national program.

- The subcommittee should enable DOE to develop and execute a fusion program that involves both U.S. industry and international collaborators in an effort to provide fusion as an option for future generations.